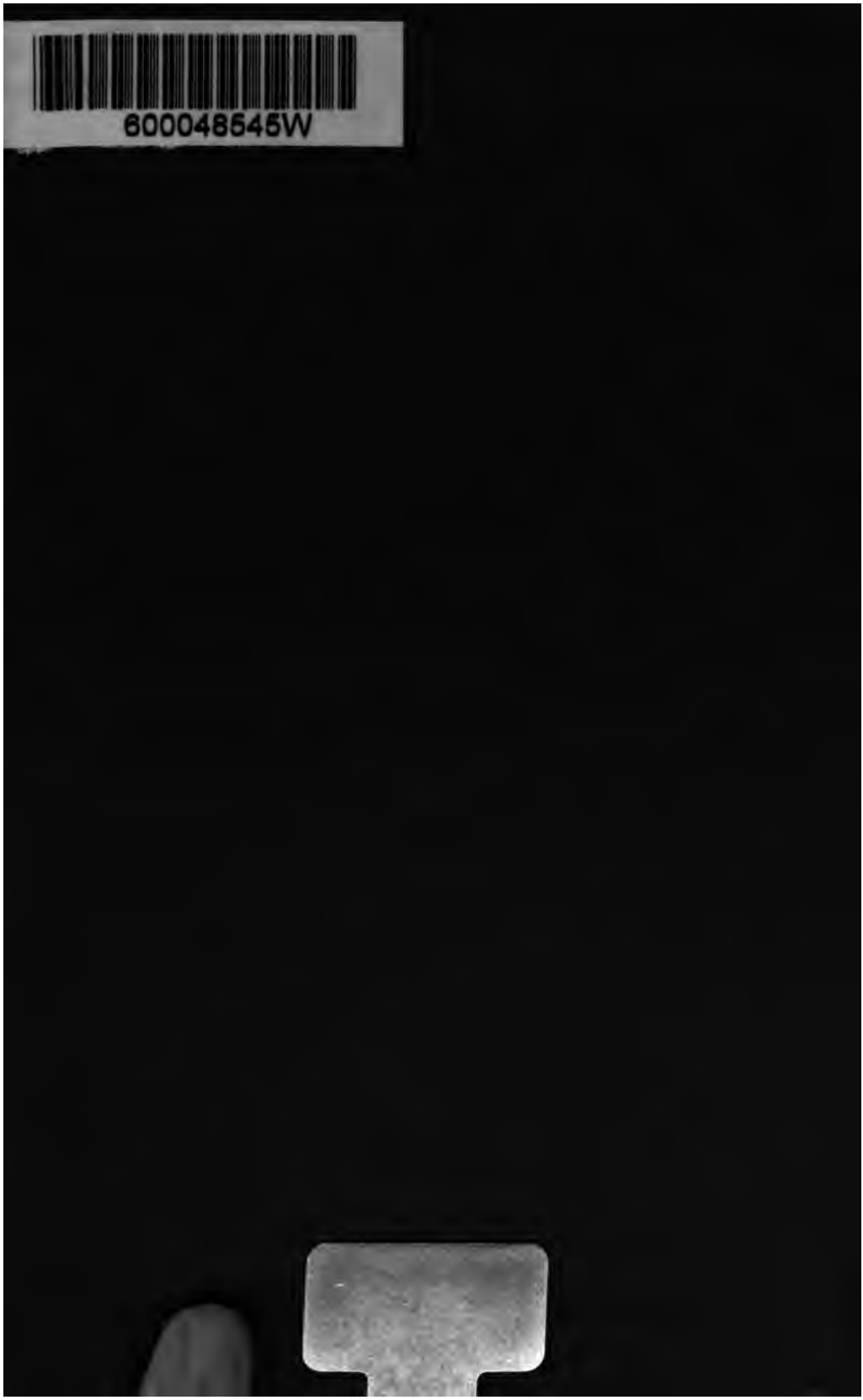


STUDIES

OF

VENUS-TRANSITS

AN INVESTIGATION OF THE CIRCUMSTANCES
OF THE TRANSITS OF VENUS
IN 1874 AND 1882

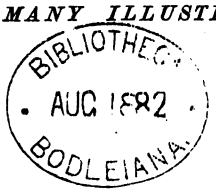
ORIGINALLY FORMING PART OF

'THE UNIVERSE AND COMING TRANSITS'

BY

RICHARD A. PROCTOR

WITH MANY ILLUSTRATIONS



LONDON
LONGMANS, GREEN, AND CO.
1882

THIS VOLUME contains simply those sheets of the work originally published under the title 'The Universe and the Coming Transits,' which were separated from that treatise when it was decided to publish the 'Universe of Stars' as a separate work. Portions are now necessarily out of date; but all that relates (1) to the transit of 1882, and (2) to the comparison between the transits of 1882 and 1874, remains as trustworthy now as when it was originally published in the Monthly Notices of the Royal Astronomical Society.

RICHARD A. PROCTOR.

LONDON: *May* 1882.

THE COMING TRANSITS.

THE TRANSIT OF VENUS IN 1874.

On account of the important bearing of the transits of Venus upon the problem of the Sun's distance, men of science are looking anxiously forward to the two transits which occur in the present century. Although the later of the two will not take place for thirteen years, its circumstances have already been examined. Indeed both transits were subjected to examination by the Astronomer Royal so far back as 1857; and since then he has continued to put forward from time to time the considerations which have suggested themselves to him as his examination of the subject proceeded. Early in the inquiry he expressed the opinion that the method founded on the observed differences of the transit's duration, as seen from opposite points of the Earth's surface—which method had been the sole one employed in the treatment of the transit of 1769—is wholly inapplicable to the transit of 1874; and he suggested another method of utilising that transit,—a method less perfect in itself, more difficult (astronomically) to carry out, and involving processes of preparation essentially different from those which would be required under the other method. To the preparations thus called for, astronomers and geographers have hitherto, I believe, solely confined themselves.

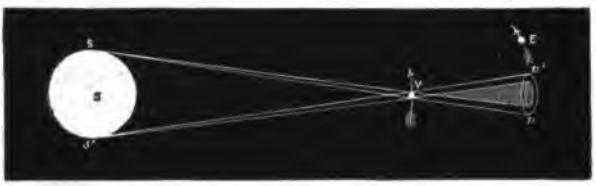
Having had occasion to examine the reasoning of the Astronomer Royal, and to test the conclusions he had arrived at, I have been led to form a totally different opinion of the value of the transit of 1874, so far as the simpler method of observation is concerned. I have found that, if consideration be made of internal contacts—the only phenomena on which estimates of the Sun's distance have ever been founded—the actual difference of duration which can be made available in 1874, is about 35m. or 36m., as against an outside value of 28m. in 1882, and an actual observed maximum of difference of 23½m. in 1769.

I am sensible that mere magnitude of observed difference is not

the sole point on which the value of a transit depends. The rate at which the planet crosses the Sun's limb is an almost equally important subject of consideration. I shall be able to show that when this point is dealt with in the manner most unfavourable to my case, the value of the transit of 1874 yet remains superior to that of the famous transit of 1769 (as actually utilised), and scarcely inferior to the most favourable estimate which can be formed of the transit of 1882. Therefore, remembering the importance which has been always attached to the observations made in 1769, and the immense advances since made in the construction of instruments and in observing-skill, we cannot look upon the transit of 1874 as otherwise than highly valuable.

If we briefly consider the general nature of a transit, we shall be the better able to define the circumstances on which the value of any particular transit depends.

FIG. 1.



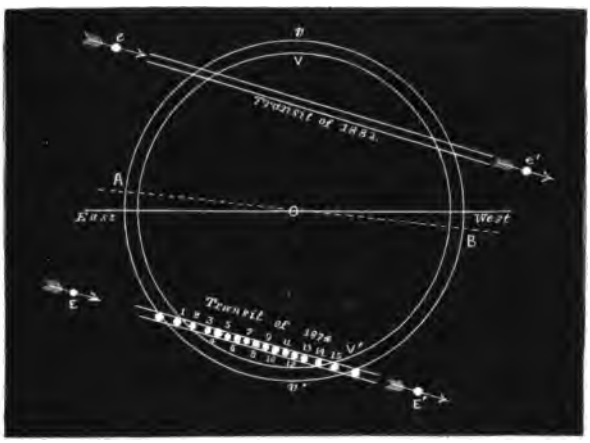
In fig. 1 let S represent the sun, V Venus, and E the Earth, the plane of the paper representing the plane of the ecliptic, so that the path of Venus (supposed to be near one of her nodes) must be conceived as inclined rather more than 3° to the plane of the paper. The arrows show the direction in which the two planets move.

Conceive the Sun and Venus enveloped by two double cones, one, $s\ v\ s'\ v'$, having its vertex inside, the other having its vertex outside, the orbit of Venus. These cones have a common axis, namely, the line joining the centres of Venus and the Sun. Now it is clear that as Venus with her more rapid motion sweeps round the Sun, the accompanying cones must overtake the Earth (situated as shown in the figure), and that as they sweep onward, the Earth will pass through them. As this takes place, there will occur the following eight phenomena in the given order: The forward part of the outer cone will reach (i) the nearer, then (ii) the farther side of the Earth's globe; the corresponding part of the inner cone will reach (iii) the nearer, then (iv) the farther side of the Earth's globe;

next, the backward part of the inner cone will reach (v) the nearer, and (vi) the farther side of the Earth; and lastly, the corresponding part of the outer cone will reach (vii) the nearer, and (viii) the farther part of the Earth. And these several events will readily be seen to correspond to the occurrence of:—

(i) Most accelerated external contact at ingress.
(ii) „ retarded „ „
(iii) Most accelerated internal contact „
(iv) „ retarded „ „
(v) Most accelerated „ at egress.
(vi) „ retarded „ „
(vii) Most accelerated external contact
(viii) „ retarded „ „

Fig. 2.



In fig. 2 the actual nature of the Earth's passage through the cones is illustrated, as exactly as possible, to scale. In this figure the large circles represent the sections of the outer and inner cones where they cross the Earth's orbit. For convenience we consider these circles to be at rest, and examine only the effects of the Earth's relative motion. B O A is parallel to the ecliptic. As the Earth is, in reality, moving from right to left in a direction parallel to B O A, and with a less rapid motion than that of the two large circles, it is clear that the Earth's relative motion is from left to right. Also, as the circles are crossing A O B from south towards north (for Venus

is at an ascending node both in 1874 and 1882) it is clear that the Earth has a relative motion from north to south. The pairs of lines marked with arrows show the actual direction of the Earth's relative motion (the small circles representing the Earth in various parts of her passage). Calculation shows that this relative motion is such that a central transit would occupy nearly eight hours. The pairs of lines are inclined to A O B at about $9\frac{1}{2}$ degrees; and 14 degrees to the east and west line.

The actual path of the Earth across the circles in 1874 and 1882 is indicated by the lower and upper sets of triple lines, respectively.

In considering the circumstances of the transit of 1874, I dismiss all consideration of the phenomena marked (i), (ii), (vii), and (viii) in the above table. But besides the four remaining phenomena, I take into account the passage of the Earth's *centre* across the circles, because Mr. Hind's elements are calculated only for these phenomena, and without considering them I should have been unable to test the accuracy of my own conclusions by comparing them with his results.

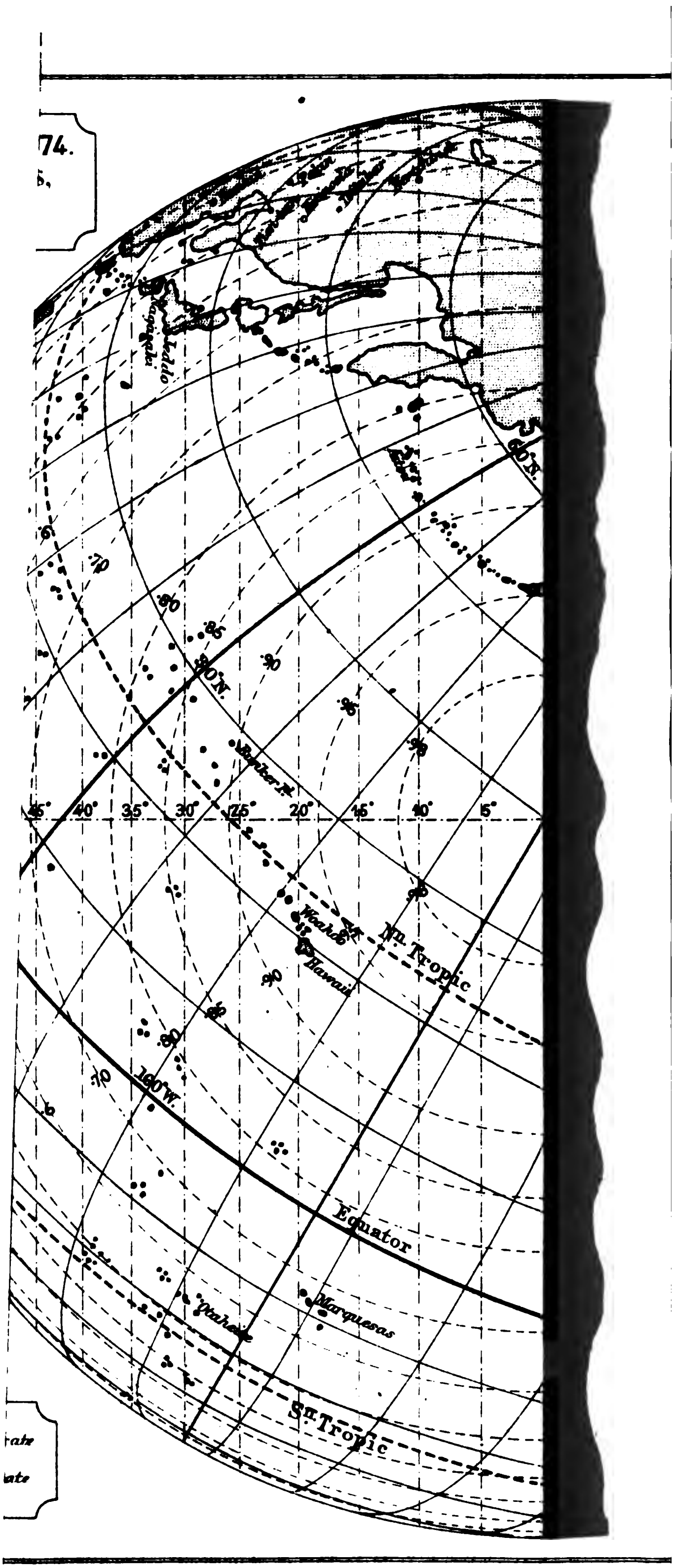
Thus the eight phenomena corresponding to the positions of the earth, numbered 1 to 8 in fig. 2, correspond to—

(1) External contact for the Earth's centre at ingress.
(2) Internal contact most accelerated at ingress.
(3) „ as seen from the Earth's centre.
(4) „ most retarded at ingress.

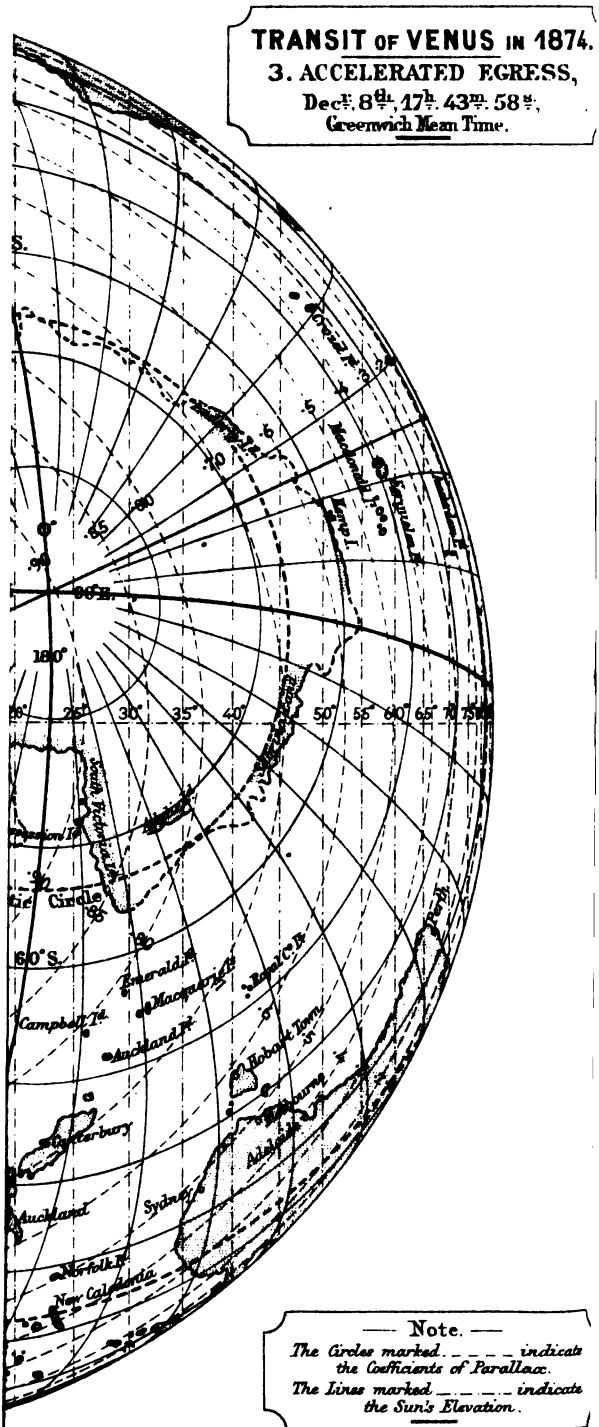
and the four corresponding phenomena at egress, viz.—

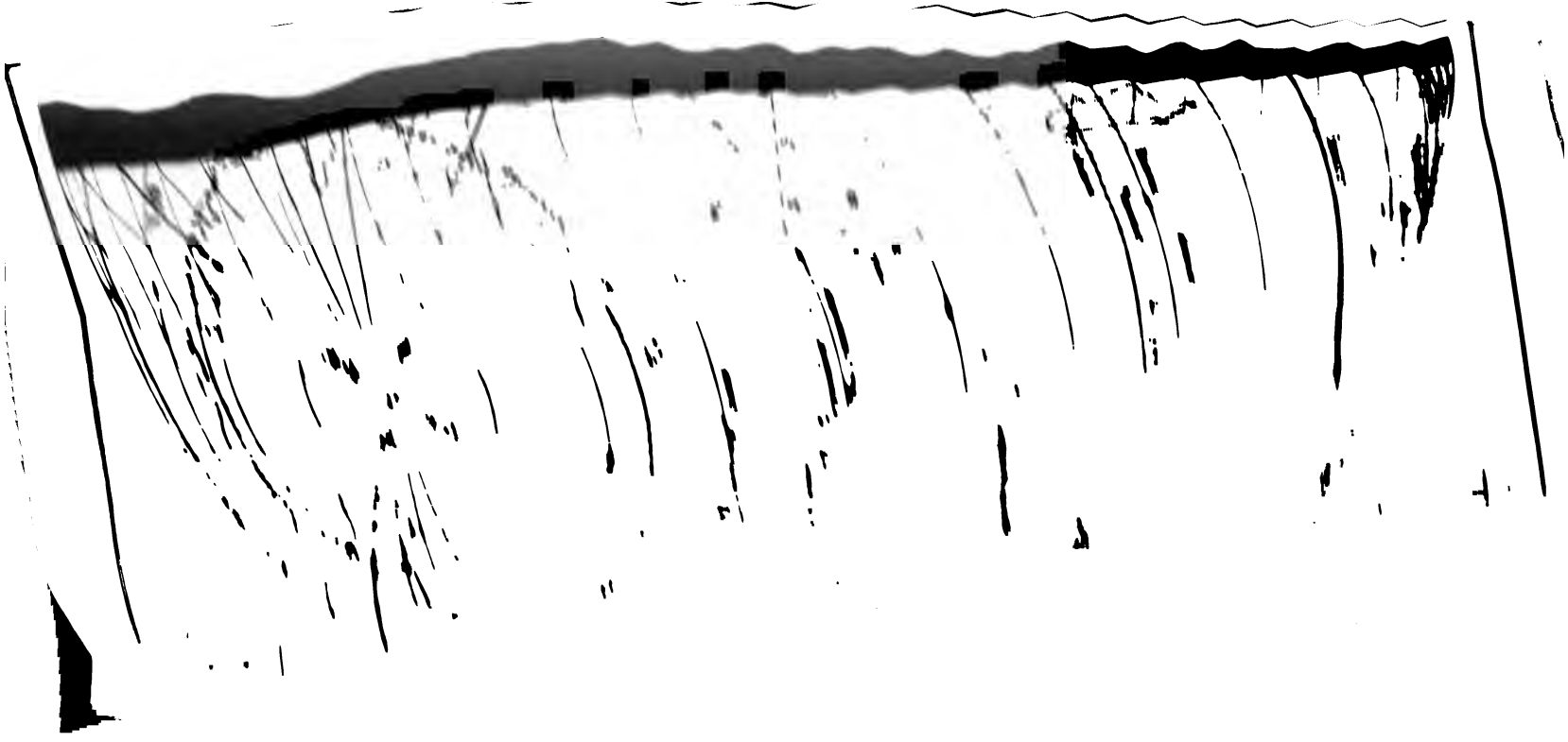
(5) Internal contact most accelerated at egress.
(6) „ as seen from Earth's centre.
(7) „ most retarded at egress.
(8) External contact for the Earth's centre at egress.

The treatment applied to these phenomena has been the following:—Taking Mr. Hind's epochs for the external contacts at ingress and egress, I have thence calculated all the remaining epochs and the position-angles; the exact agreement of my estimates of such of these elements as Mr. Hind has calculated, with the results he has obtained, has sufficed to establish the correctness of the mode of operation. Thus has been formed the following table for the phases mentioned above.



74.
Equator
Nn. Tropic
Sn. Tropic
Marquesas
Otaheite
Hawaii
Woahoo
30 N.
160 W.
45°
40°
35°
30°
25°
20°
15°
10°
5°


TRANSIT OF VENUS IN 1874.
3. ACCELERATED EGRESS,
Dec.r 8d, 17h. 43m. 58s.,
Greenwich Mean Time.
S.
90°E.
180°
20°
25°
30°
35°
40°
45°
50°
55°
60°
65°
70°
75°
Circle
60° S.
Campbell I.
Auckland I.
Macquarie I.
Emerald I.
Royal C.
Hobart Town
Melbourne
Adelaide
Sydney
Auckland
Norfolk I.
New Caledonia
Note.
The Circles marked _ _ _ _ indicate the Coefficients of Parallax.
The Lines marked _ _ _ _ indicate the Sun's Elevation.



THE COMING TRANSITS.

It was the consideration of these circumstances which led the Astronomer Royal to pronounce the transit of 1874 altogether unfit for the purpose of observing the durations of transit, as seen from opposite parts of the Earth's surface. And he suggested that four sets of observers should be sent to watch each of the four phenomena,—accelerated and retarded ingress and egress; and that by determining the exact longitudes of their stations, and so (with the aid of exact chronometers) learning the exact Greenwich time of each phenomenon, the transit might be rendered available through the comparison of the results *inter se*. Clearly the elements of difficulty and the probability of error are seriously increased in this method as compared with one which practically requires but the simple estimate of duration, and scarcely admits of being affected by chronometer errors. However, let us note that by this method an observed difference of at the outside 24m. might be obtained;—not more, because the Sun cannot be observed when too close to the horizon, and because also of the difficulty of finding suitable stations.

Now let us see what can be done towards the utilisation of the transit of 1874 by the simpler method:—

Suppose the northernmost station taken in latitude 60°, that is, along the uppermost parallel in the figure. As the whole duration of transit is but about four hours, and day lasts about six hours in this latitude on December 8, we may take a place two hours on the left of the central meridian in Plate XV., knowing that the same place will be (at the end of transit) two hours to the right of the central meridian in Plate XVI.; and at one epoch the Sun will be one hour risen, at the other one hour from setting. Doing this we find that the station (which lies in Siberia, not far from Lake Baikal) falls in Plate XV.) on the sixth cross-line from the centre, and in Plate XVI. above the tenth cross-line. In other words, the transit as seen from this spot exceeds the mean by $(6+10\frac{1}{4})$, or $16\frac{1}{4}$ minutes.

Next for the southern station. Here we have a wide choice. If we put our observer on Petra Island (a place probably very little suited for astronomical observations) we get (from Plate XV.) ingress retarded by 8m., and (from Plate XVI.) egress accelerated by 12m., or in all the duration of transit falls short of the mean by 20m. If we take the place marked out by the Astronomer Royal for observing the transit of 1882, a place near Repulse Bay, in east longitude 105°, we get ingress retarded by 9m. and egress

accelerated by $9\frac{1}{2}$ m., or in all transit shortened by $18\frac{1}{2}$ m. If we take Victoria Land, in south latitude 70° (say), and east longitude 172°, we get ingress retarded by 6 m. and egress accelerated by $11\frac{1}{2}$ m., or in all transit shortened by $17\frac{1}{2}$ m. If we take Enderby Land, in east longitude 50°, we get ingress retarded by $11\frac{3}{4}$ m. and egress accelerated by $8\frac{1}{2}$ m., or in all transit shortened by $20\frac{1}{4}$ m. These four southern stations, combined with the northern station before considered, give a total difference of duration of $36\frac{1}{4}$ m., $34\frac{1}{4}$ m., $33\frac{1}{4}$ m., and $36\frac{1}{2}$ m. respectively. Also, as it would not be well to trust to a single northern station, it may be noticed that any part of the nearly circular region extending from Lake Baikal to Saghalien, and from north latitude 40° to north latitude 60°, might be used for observing the increased duration without important disadvantage as compared with the station already considered. Also, Crozet Island, Kerguelen's Land, and other parts of the Antarctic continent besides those considered, give abbreviated transits of considerable value. Thus for Crozet Island the abbreviation is no less than 17 m.; for Kerguelen's Land, 16 m. Even Macquarie Island, Royal Company Island, Hobart Town, and parts of New Zealand, might serve as useful subsidiary stations.

And now to compare the value of the transit of 1874 with that of 1882. We see that by the method of durations we get a difference of more than 36 m., whereas the maximum difference is $50\frac{1}{3}$ m. The Astronomer Royal has shown that for the transit of 1882 it is possible to take positions for observation (not by any means more favourable than those above considered) which give at the outside a difference of duration bearing to the maximum the proportion of 341 to 400. The maximum difference in the case of the transit of 1882 is only 32m. 48s., in place of 50m. 12s. as in 1874. Reducing 32m. 48s. in the proportion of 341 to 400, we obtain the period 27m. 57s. in place of the difference of $36\frac{1}{2}$m. which the most favourable situations in 1874 will give.

If we assume that the value of a transit is not to be estimated according to the magnitude of the observable difference, because the rate with which the planet crosses the Sun's limb is diminished in exactly the same proportion, and the error of observation correspondingly increased, we have the relative values of the transits of 1874 and 1882 as

$$\frac{36\frac{1}{2}}{50\frac{1}{3}} \text{ to } \frac{28}{32\frac{3}{4}}$$

or almost exactly as 6 to 7. But this extreme result, although as

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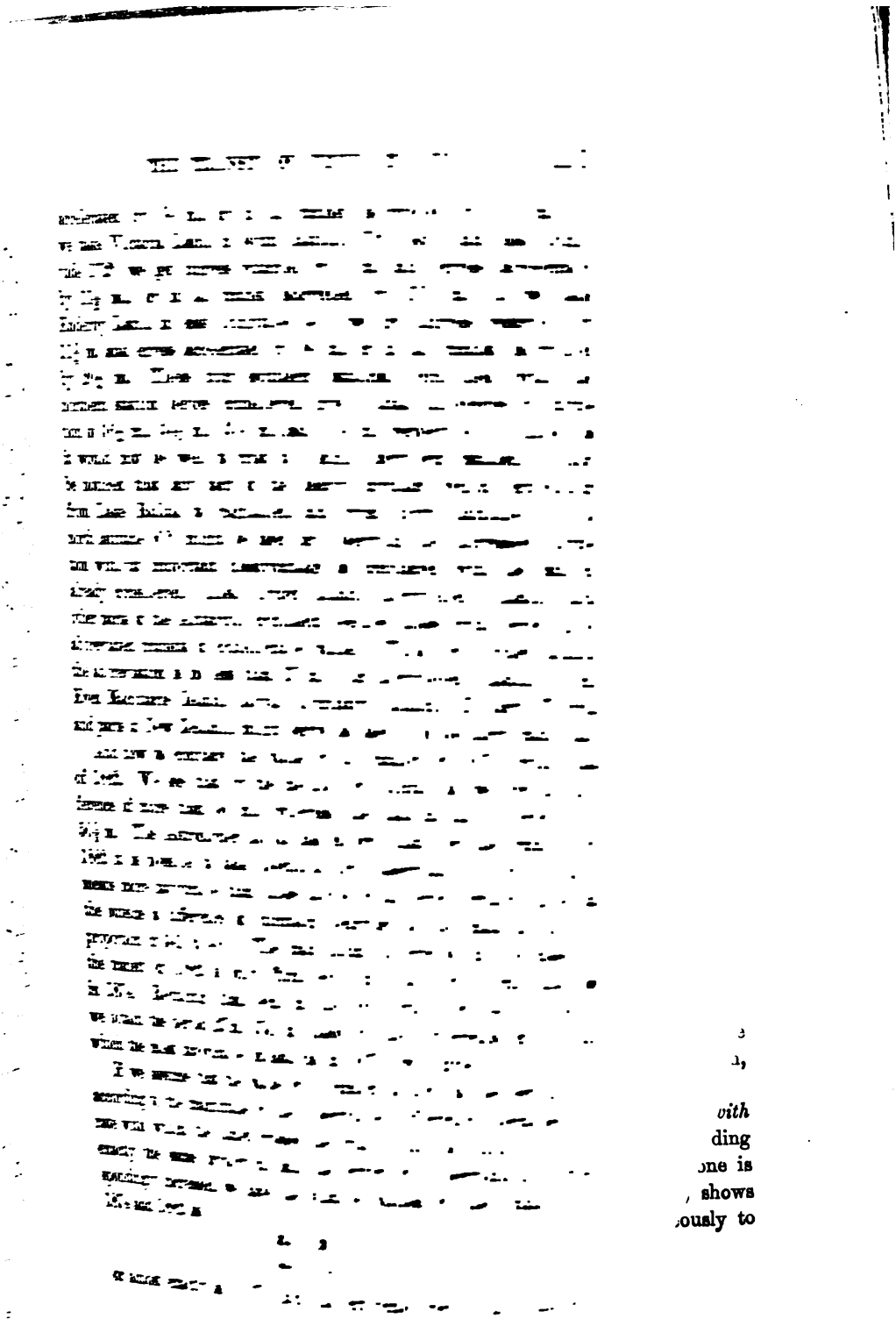


it stands it is altogether opposed to the theory of the utter valuelessness of the transit of 1874, is obtained on an assumption which is unsupported by evidence. Mr. Stone has shown that the formation and breaking of the black ligament connecting Venus with the Sun at the true moment of internal contact is an instantaneous phenomenon in favourable weather. In unfavourable weather the error in the observation of this phenomenon should depend rather on atmospheric causes—the length of the periods of atmospheric disturbance, and so on—than on the rate of the planet's separation from the Sun's limb. If this is so, the transit of 1874 is superior to that of 1882 in the proportion of 36½ to 28, or more than 9 to 7. If the truth lies between these extremes, the transit of 1874 may be fairly taken to have a value bearing to that of 1882 a proportion midway between 6 : 7 and 9 : 7; that is, the proportion of 15 : 14.

In any case no doubt can remain that the transit of 1874 is highly valuable, when dealt with in reference to the mode of observation we have been considering; and it seems clear that when all the difficulties and all the sources of error involved in the second method are duly considered, the simple method, founded on observed differences of duration, is to be held altogether more likely to give satisfactory results. I believe, therefore, that such preparations as geographers are already thinking of with reference to the choice of suitable southern stations for observing the transit of 1882 ought at once to be undertaken in connection with the transit of 1874.[1]

From the *Quarterly Journal of Science* for July 1869.

[1] Since the above was written the subject of the coming transits has been considered by Mr. Stone, than whom no one is better qualified to pronounce authoritatively on the principles which should guide us in utilising those phenomena. He is of opinion that observations made when the Sun has a less elevation than 10° would be altogether useless. This principle enables me to considerably augment my estimate of the relative superiority of the transit of 1874, with reference to the simpler mode of observation. In fact, the only southern stations which had seemed suitable in 1882 must at once be rejected; and thus we may say of that transit what had been said of the other, that the simpler mode 'fails totally' with respect to it. On the other hand, the value of the transit of 1874 is scarcely at all affected by the application of the principle.

Mr. Stone considers the superiority which I have ascribed to the simpler method to have a real existence, but to be so slight (the values of the two modes being as 6 to 5) as to be unimportant. This is just; but the distinction between this view and the imagined total failure of the method seems not the less to require attention.

CORRECTIONS OF THE ASTRONOMER ROYAL'S STATEMENTS.

It is shown in the preceding article how the differences which exist between my results and those obtained by the Astronomer Royal, result from an adoption of a comparatively rough method of dealing with the problems involved. I now proceed to indicate the extent of these corrections, as also of those which have to be applied to the somewhat more exact work of the French astronomer Puiseux.

I first give a general summary of the changes under three heads:—

1. *The application of Delisle's method of absolute time differences.* The relative as well as the absolute values of many stations are affected. Some which had hitherto appeared unsuitable are found to be unobjectionable. Others which seemed good appear unfit. In other cases the relative values of two stations are so affected that the results of a comparison between them are directly reversed. Lastly, many stations not hitherto thought of in connexion with the transit are found to be well suited for the application of Delisle's method.

2. *The comparison between Delisle's and Halley's methods.* Halley's method is found not merely to be applicable with advantage, which is all that can be said of it when central passages are considered, but to be superior to Delisle's,—slightly, when reference is made only to such stations as had been hitherto dealt with, noticeably when Antarctic stations are made use of.

3. *The comparison between the Transits of* 1874 *and* 1882 *with reference to Halley's method.* This comparison, conducted according to the principles laid down by Mr. Stone (than whom no one is better entitled to pronounce authoritatively on such points) shows that Halley's mode may be applied much more advantageously to the transit of 1874 than to that of 1882.

The results to be now brought into comparison for the sake of forming an estimate of the effect of phase, parallax, and the equation of time, upon the values of various stations, are these :—

(1) Airy's, derived from the consideration of central passages, as supposed to be seen from the earth's centre, with the position-angles corresponding to external contact.
(2) Puiseux's, derived from the same phase, similarly seen, with the position-angles corresponding to central passage.
(3) Proctor's, derived from the consideration of internal contacts, as seen from the stations themselves, and with the position-angles corresponding to the phase so seen, and correction being made for the equation of time.

The following table exhibits the position-angles and epochs (for ingress) corresponding to these three orders of result :—

	Position-angles			Epochs		
	Airy	Puiseux	Proctor	Airy	Puiseux	Proctor
	°	° ′	° ′	h m	h m	h m
Accelerated Ingress	131	133 29	133 56	14 0	13 55	14 4
Mean Ingress	131	133 29	136 28	14 0	13 55	14 16
Retarded Ingress	131	133 29	139 29	14 0	13 55	14 29

In preparing the columns giving my own results in the accompanying tables, I have made use of the six orthographic maps forming Plates XIII., XIV., XV., and XVI. These were constructed with every precaution to ensure accuracy. The intersection of longitude-lines and latitude-parallels (to every 10°) were separately obtained by a double process of construction, and in all critical cases further tests were applied. In all, the construction of the maps involved upwards of 3,000 measurements. The results indicated in the maps have been also abundantly confirmed since the maps appeared, by the calculations and chartings made by others, and especially by the beautiful maps published by the American Government.

The six maps include four quarter-spheres, exhibiting the solar elevations and the coefficients of parallax.[1] The other two exhibit the Earth as supposed to be seen from the Sun at ingress and egress (*mean*, and for internal contacts). In these the solar elevations are indicated by circles, and in place of 10 parallactic parallels, corresponding to the parallactic circles in the other map, there are laid down parallactic lines corresponding to minute intervals (the line across the Earth's centre being taken as a zero-line). These

[1] The circles marked ·9, ·8, &c., pass through all the points at which ingress or egress is accelerated or retarded (as the case may be) by $\frac{9}{10}$ths, $\frac{8}{10}$ths, &c. of the maximum acceleration or retardation.

lines are not parallel, but separately constructed for. Thus their indications differ somewhat from those derived from the parallactic circles in the other maps, which are laid down on the supposition (not strictly correct) that the outline of the penumbra of Venus travels parallel to itself across the face of the Earth. This will account for a slight want of correspondence between the second and third columns under my own name in the following tables: the third gives the correct effect of parallax. It will be noticed, however, that the difference is always trifling in the case of places suitable for the application of Delisle's method.

A further correction, but one of small importance, would result from the consideration that the apparent outline (supposed to be seen from the Sun) of that part of Venus's penumbra which traverses the Earth is not a straight line, but part of a large circle. Thus the actual outline on the Earth's surface is not part of a circle. It follows that the parallactic curves in the four quarter spheres ought not to be circular, and the parallactic lines across the other two maps ought to be curved, the direction of their curvature being turned in the direction towards which the shadow is moving for ingress, and the reverse for egress. All the corrections due to this cause are minute, and attain their greatest values at places not suitable as stations either for the application of Delisle's or Halley's method.[1]

A correction has been applied to the columns under my name corresponding to the fact that the maps are severally constructed for a single epoch, while the events to which they relate occupy several minutes.

[1] This correction has been taken into account in constructing the stereographic chart forming Plate XX.

TABLE I.—*Accelerated Ingress.*

Station	Sun's Elevation			Coefficient of Parallax		Acceleration	
	Airy	Puiseux	Proctor	Airy	Proctor	Puiseux	Proctor
	°	°	°			m	m
Woahoo . .	22·5	...	19·8	·92	·93	...	11·2
Hawaii . .	22·3	21·5	19·7	·92	·92	10·3	11·1
Aiton I. (Aleutian)	12·0	...	10·8	·80	·84	...	10·3
Marquesas I. .	20·0	23·1	17·7	·71	·66	7·5	7·9
Mouth of Amoor R.	15·0	...	14·0	·57	·62	...	7·6
Jeddo . . .	...	30·9	32·1	...	·50	...	6·8
Otaheite . .	34·5	34·3	29·7	·59	·54	6·1	6·4
Nertschinsk . .	...	...	10·1	...	·41	...	5·8
Tsitsikar . .	...	...	17·0	...	·43	...	5·8
Kirin-Oula . .	...	...	19·5	...	·42	...	5·7
Nagasaki . .	...	...	32·7	...	·40	...	5·3
Tientsin . .	...	...	22·2	...	·38	...	5·0
Pekin . . .	...	20·2	20·8	...	·30	3·0	4·3
Shanghai . .	...	29·5	28·5	...	·25	2·6	3·9
Nankin . .	...	...	27·1	...	·20	...	3·6
Canton . .	...	...	35·5	...	·08	...	1·6
Hongkong . .	...	...	36·2	...	·08	...	1·6

TABLE II.—*Retarded Ingress.*

Station	Sun's Elevation			Coefficient of Parallax		Retardation	
	Airy	Puiseux	Proctor	Airy	Proctor	Puiseux	Proctor
	°	°	°			m	m
Crozet I. .	9·5	...	15·0	·98	·96	...	12·6
Enderby Ld. .	...	17·3	20·0	...	·92	10·3	11·8
Kerguelen Ld. .	25·0	23·6	27·5	·91	·88	10·3	11·6
Macdonald I. .	...	27·2	31·0	...	·85	...	11·2
Kemp I. . .	...	...	30·0	...	·85	...	11·1
Bourbon I. . .	4·5	...	12·4	·93	·84	...	11·1
Mauritius . .	6·0	...	14·1	·92	·81	...	10·7
Amsterdam I. .	...	27·6	34·1	...	·77	9·8	10·3
Rodriguez . .	11·5	...	19·0	·89	·76	...	9·9
Sabrina Ld. . .	...	...	45·0	...	·73	...	8·2
Adelie Ld. . .	...	...	45·0	...	·50	...	6·8
Possession I. .	...	36·4	38·5	...	·46	5·0	6·0
Perth (Aust.) .	...	...	65·0	...	·38	...	5·3
Royal Co. I. . .	...	...	62·0	...	·32	...	4·5
Madras . .	12·5	...	21·0	·47	·25	...	4·0
Bombay . .	4·5	...	12·5	·44	·22	...	3·8
Macquarie Ld. .	...	...	52·0	...	·25	2·9	3·5
Hobart Town .	...	70·0	67·0	...	·20	...	2·8
Adelaide . .	...	...	75·0	...	·18	...	2·5
Melbourne . .	...	...	75·0	...	·13	...	2·2

TABLE III.—*Accelerated Egress.*

Station	Sun's Elevation			Coefficient of Parallax		Acceleration	
	Airy	Puiseux	Proctor	Airy	Proctor	Puiseux	Proctor
	°	°	°			m	m
Possession I.	...	23·8	25·0	...	·89	10·3	11·4
Adelie Ld.	...	...	34·0	...	·83	...	10·6
Campbell I.	...	...	26·0	...	·79	...	10·3
Emerald I.	...	...	30·0	...	·78	...	10·3
Macquarie I.	24·0	...	32·0	·83	·75	...	9·8
Chatham I.	11·5	...	16·0	·87	·75	...	9·8
Canterbury (N.Z.)	19·0	...	22·5	·81	·71	...	9·3
Wellington	17·0	...	20·0	·81	·70	...	9·2
Sabrina Ld.	...	...	43·0	...	·70	...	9·2
Enderby Ld.	...	41·0	39·0	...	·65	6·9	8·5
Royal Co. I.	...	...	42·0	...	·64	...	8·5
Auckland	15·0	14·4	19·2	·78	·64	8·6	8·5
Kemp. I.	...	...	51·0	...	·57	...	7·6
Hobart Town	36·0	36·0	40·0	·66	·57	7·5	7·6
Melbourne	38·0	...	43·0	·58	·48	...	6·6
Sydney	33·0	...	37·2	·59	·48	...	6·6
Adelaide	...	...	47·8	...	·40	...	5·8
Kerguelen Ld.	...	60·5	57·1	...	·40	...	5·0
Crozet I.	...	...	47·5	...	·34	...	4·2

TABLE IV.—*Retarded Egress.*

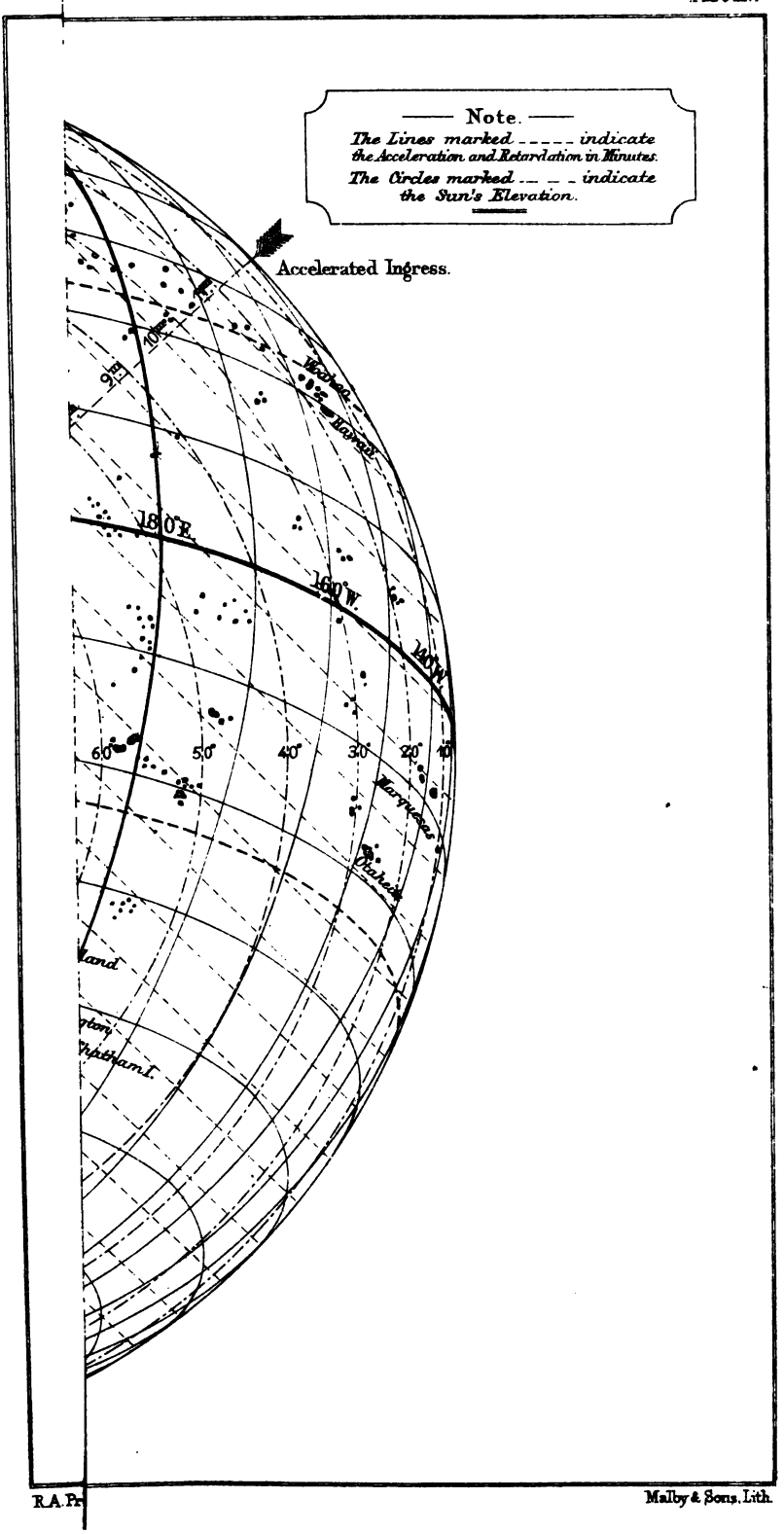
Station	Sun's Elevation			Coefficient of Parallax		Retardation	
	Airy	Puiseux	Proctor	Airy	Proctor	Puiseux	Proctor
	°	°	°			m	m
Orsk	12·0	...	12·5	·98	·98	...	11·8
Omsk	12·5	...	11·5	·96	·97	...	11·7
Astracan	12·4	...	12·0	·97	·97	...	11·6
Aleppo	14·2	...	14·6	·91	·89	...	10·5
Peshawur	...	...	31·5	...	·85	...	10·3
Alexandria	13·0	...	14·0	·86	·84	...	10·0
Suez	16·0	17·6	16·1	·85	·83	9·4	9·8
Nertschinsk	...	...	10·1	...	·81	...	9·8
Delhi	...	...	38·0	...	·78	...	9·4
Tsitsikar	...	...	12·0	...	·72	...	8·7
Bombay	...	...	45·0	...	·70	...	8·5
Pekin	...	21·2	21·0	...	·70	7·7	8·6
Kirin-Oula	...	...	14·0	...	·69	...	8·4
Tientsin	...	...	17·1	...	·68	...	8·4
Calcutta	...	...	45·3	...	·68	...	8·2
Aden	...	...	36·0	...	·66	...	7·8
Nankin	...	...	27·0	...	·62	...	7·6
Madras	...	...	52·0	...	·61	...	7·4
Shanghai	...	26·1	26·0	...	·57	6·2	7·2
Canton	...	...	37·0	...	·52	...	6·6
Hongkong	...	...	37·0	...	·50	...	6·5

It will be seen, on a comparison of these columns, that the effects of the change of phase are in some cases important. The coefficients of parallax are affected in several instances by more than 0·1, and in two cases by 0·22. In the cases of Crozet Island (Table II.) and Chatham Island (Table III.) solar elevations are so improved that these stations, which would have to be rejected if central passage were considered, are shown to be well suited for the observation of internal contacts. The diminution of all the coefficients in Table III., through the change of phase, has an important influence on the value of Delisle's method, so far as egress observations are concerned. It is important to notice, also, that in the columns under my name in Tables III. and IV. many stations not hitherto recognised as available are included among the best places for observing egress. The Indian stations in Table IV. are too valuable to be neglected. Peshawur is better even than Alexandria; Delhi is not inferior to the latter station (when solar elevation is considered as well as coefficient of parallax). Bombay, Calcutta, and Madras are also excellent. It may be noticed also that Bombay and Madras, which, when considered with reference to central passage, had seemed suitable places for the observation of retarded ingress, are found to have so poor a coefficient of parallax when reference is made to internal contacts, that it would be useless to observe ingress there (so far at least as the application of Delisle's method is concerned).

Of course, it will be impracticable for this country to send observers to more than a certain number of stations. But it is not unlikely that besides Russia, France, and England (the only countries specially concerned in the transit of 1874), other nations may care to take part in the solution of the noble problem of determining the Sun's distance; and thus it seems advisable that all the stations where there will be any chance of obtaining useful observations, should be tabulated as nearly as possible according to their true relative values.

As regards the comparison between Delisle's and Halley's method for the transit of 1874, I may remark that M. Puiseux's results seem to have been somewhat misinterpreted. He does not anywhere speak of Halley's method as the best, but simply states that he can see no reason why it should not be applied: nor do his figures establish the superiority of Halley's method.

I believe I shall be able to show, however, that there is at least a possibility that Halley's method may be so applied to the transit of



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1874 as to give absolutely the best means of determining the Sun's distance available before the transit of 2004 (about the circumstances of which little is known). It only requires that the same energy should be devoted (either by England or some other nation) to the coming transit, which has been called for in relation to the transit of 1882. There will also be the same chance of failure in one case as in the other.

To prove the justice of these views I point to Nertschinsk (or its neighbourhood) as a suitable station for observing the lengthened transit, and the neighbourhood of Enderby Land as a proper station for observing the shortened transit. The above tables give for the former station a lengthening of ($5{\cdot}7^m + 9{\cdot}8^m$) or $15{\cdot}5^m$; and for the latter a shortening of ($11{\cdot}8^m + 8{\cdot}5^m$) or $20{\cdot}3^m$. The total difference of duration is thus shown to be $35{\cdot}8^m$. Setting against this the best case for Delisle's method (Woahoo from Table I. and Crozet Island from Table II.), we get the total difference of absolute time, ($11^m{\cdot}2 + 12^m{\cdot}6$) or $23^m{\cdot}8$. And the relative values of the methods (in these cases the most favourable for each) are given by the formula

$$\frac{\text{Halley's}}{\text{Delisle's}} = \frac{(35{\cdot}8)^2}{(23{\cdot}8)^2} \frac{\{(4{\cdot}28)2 + (1)^2\}^*}{2(4{\cdot}28)^2} = \frac{2477\dagger}{2075} \text{ approximately;}$$

and in this ratio, independently of all the other advantages which it presents, does Halley's method surpass Delisle's. By taking Kemp Island, Adelie Land, Victoria Land, Crozet Island, or Kerguelen's Land for the southern, and Tsitsikar, Kirin-Oula, or Tientsin, for the northern stations, we get differences of duration ranging from 33^m to $30{\cdot}6^m$, corresponding (according to Mr. Stone's formula) to a range of from 24^m to $22{\cdot}2^m$, in the case of Delisle's method. There is little chance of the latter method being applicable with a difference

* Here I have used the values given in Mr. Stone's paper, 'Monthly Notices of the Astronomical Society,' April 8, pp. 251, 252. He would have obtained a larger value for e had he calculated for internal contacts. This would, of course, have been a change unfavourable to my case. But against this I set the fact that his careful examination of the circumstances of internal contacts will tend to render the probable errors much smaller (relatively) in 1874 than they were in 1769, when the observers had very vague notions of the phenomena they were to expect and to watch.

† It would not be correct to reduce the number of minutes 35·8 in a certain proportion, and take the excess over $23{\cdot}8^m$, as measuring the superiority of Halley's mode. The proper way of treating the question is to indicate the relation between the two modes by a ratio, as above.

greater than the lowest of these values; and the highest is a difference which the best stations will not give by Delisle's method.

When it is remembered that Halley's method is so much the simpler, and that stations selected with reference to it give a double chance of at least a useful observation, the above considerations seem to decide the question of the relative values of the two methods in 1874.

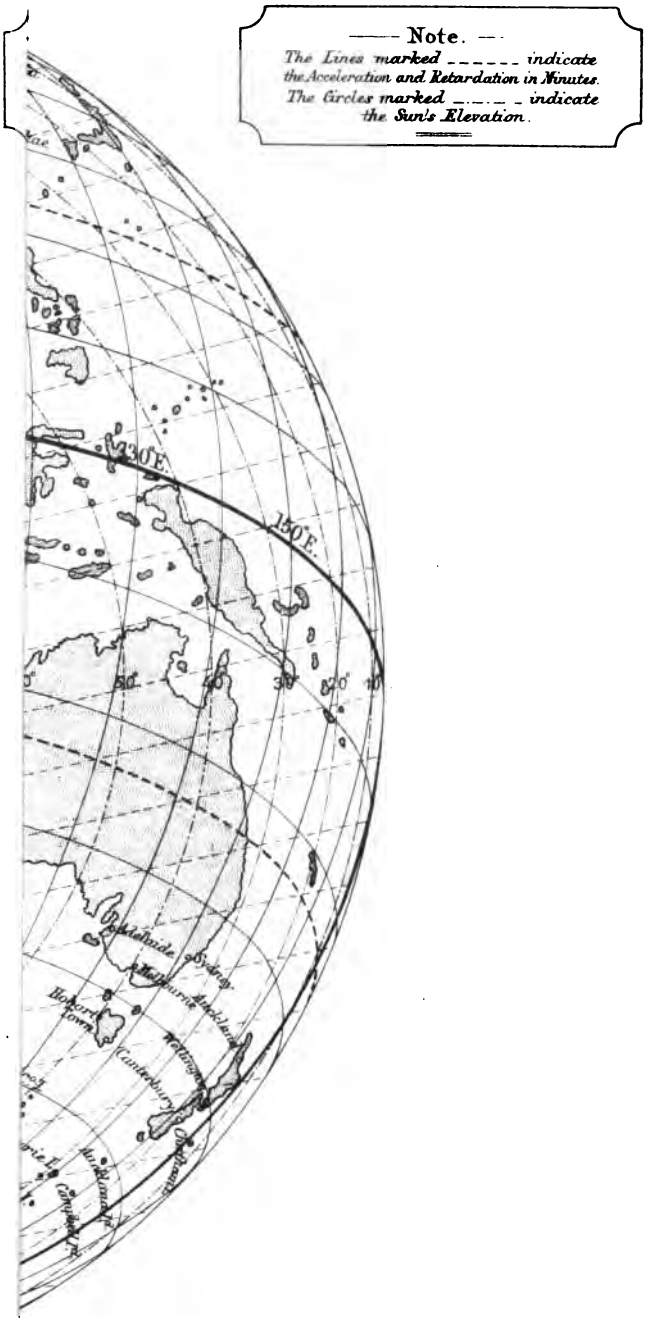
Lastly, as to the relative values of the transits of 1874 and 1882, considered with reference to Halley's method.

Mr. Stone's remark, that the choice of stations is limited to those at which the Sun will have an elevation of at least 10°, reduces the maximum available difference of duration in 1874 from $36\frac{3}{4}^{m}$ (the value I had before mentioned) to $35\frac{3}{4}^{m}$. But the transit of 1882 is much more seriously affected. The suggested station near Sabrina Land must be rejected at once. And although the suggested station near South Victoria Land corresponds to an elevation of 10° at ingress (see Airy's map 5, 'Monthly Notices' for December 1868), there is no accessible spot in that neighbourhood which will give any such elevation. At Possession Island the Sun's elevation will not be much more than 5° at ingress; and at Coulman Island, the most southerly station which Antarctic seamen hope to reach, the Sun's elevation will be but 7°; and even if these islands were suitable, they give a difference of duration perceptibly less than that which I had dealt with in my former paper.

I must add that I had fully taken into account the difference in the clinging of the disc of Venus to the Sun's limb in 1874 and 1882. Indeed, I had adopted a considerably greater proportion than that indicated by Mr. Stone. But as the principle he lays down requires that both Sabrina Land and Victoria Land should be dismissed from our consideration in 1882—and as there is absolutely no other southern station at all comparable with these two, as far as the lengthening of the transit's duration is concerned, we seem forced to the conclusion that if Halley's method fails totally for either of the coming transits, it is for that of 1882 and not for that of 1874.

Abridged from the *Monthly Notices of the Royal Astronomical Society* for June 1869.[1]

[1] The paper is also altered in certain unimportant respects. And where in the original I wrote simply A, B, and C, I now substitute the names Airy, Puiseux, and Proctor. The time has passed when reticence as to personal identity is useful, proper, or even just.



R.A.Proc

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ON THE APPLICATION OF PHOTOGRAPHY

AS A MEANS OF DETERMINING THE SOLAR PARALLAX FROM THE TRANSIT OF VENUS IN 1874.

It is impossible to read Dr. De la Rue's account of the results of careful measurement applied to photographs of the solar eclipses in 1860 and 1868 without recognising that we have in photography, as applied to the approaching Transit of Venus, one of the most powerful available means of determining the Sun's distance. Within the last few years, solar photography has made a progress which is very promising in regard to the future achievements of the science as an aid to exact astronomy. So that doubtless, in 1874, astronomers will apply photographic methods to the transit of that year, with even greater success than we should now be prepared to anticipate. It has therefore seemed to me that the photographic observation of the coming transit merits at least as full a preliminary inquiry as either Halley's or Delisle's method of direct observation.

The result of an inquiry directed to this end has led me to the conclusion that photographers of the approaching transit should adopt for their guidance considerations somewhat different from those which have hitherto been chiefly attended to.

It is undoubtedly true, as Dr. De la Rue has pointed out, that the photographer of the transit can readily take a large number of pictures, and by combining these, can ascertain with great accuracy the path of Venus across the solar disc. And by comparing the paths thus deduced for different stations a satisfactory estimate can be formed of the solar parallax. I do not wish to suggest any departure from this course of procedure.

On the other hand, it is undoubtedly true, as Major Tennant has remarked, that the greatest effect of parallax will be obtained, for any two stations, when both stations, the Earth's centre, and the centre of Venus, are in one and the same plane. So far as those two stations are concerned, his remark is just, that it is the position

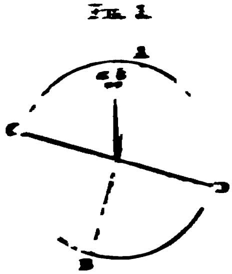
of Venus at the instant when the contacts are so assumed, and not the nearest approach of Venus to the Sun's centre, which should be compared. And further, Dr. De la Rue's statement on this, to the effect that the method in reality includes Major Tennant's, is also correct. In fact, there can be no doubt that the position of Venus at the particular instant referred to by Major Tennant can be far more exactly ascertained by a reference to the complete path of Venus for each station than from any attempt to secure exactly simultaneous photographic records at stations far removed from each other.

But it appears to me that the method I am about to suggest, according to which the whole question will be reduced to the determination of a parallactic displacement of Venus on a line through the centre of the Sun's disc, is the one by which the fullest assistance will be obtained from photography; while a source of error which has not hitherto been specially considered, will be practically eliminated.

It must be remembered that in the comparison of photographic pictures, whether for the determination of the path of Venus across the Sun's disc at a particular station, or for the comparison either of Venus's apparent position or of her path as seen from two different stations, the accuracy of the results will depend in part on the certainty with which two or more pictures may be brought into comparison by means of a fiducial line or set of lines. It seems certain that no method can be devised by which all chance of error from this source can be eliminated. The great point would, therefore, seem to be to render its effect as small as possible.

Now let us consider for a moment Major Tennant's proposition, as giving a convenient illustration of the effects of any error either in the position of the fiducial lines, or in bringing those belonging to two pictures into exact correspondence. Let fig. 3 represent the result of a comparison between two photographs of the Sun. A B and C D are fiducial cross-lines common to both pictures; *a* is the centre of Venus for one picture, *b* is her centre for the other: and on the exact measurement of *a b* depends the determination of the Sun's parallax, so far at least as these two pictures are concerned. Now it is very obvious that if the lines A B, C D, for one picture, have not been

Fig. 3.

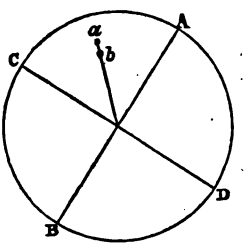


brought into perfect correspondence with those belonging to the other, the distance *a b* will be correspondingly affected. In fact, it would appear that if the usual methods for making the correspondence as exact as possible are followed, almost as large an error would be introduced through this cause alone as by errors in the measurement of *a b*, since the two processes—the measurement of *a b* and the adjustment of the sets of cross-lines—depend on the very same circumstance, the nicety, namely, with which the eye and the judgment can estimate minute quantities of about the same relative dimensions.

But now, if *a* and *b*, in place of having the position shown in fig. 3, were situated as in fig. 4, it is clear that the distance *a b* will not be appreciably affected by any small error in the adjustment of the fiducial lines.

The object, therefore, which it seems most desirable to secure is that Venus, as seen from two different stations at a particular instant, should have a relative parallactic displacement towards the Sun's centre, or as nearly towards the Sun's centre as possible. This amounts to adding to Major Tennant's conditions this further one, that the Sun's centre should be in the same plane with the two stations—or rather to making this condition a substitute for that one which requires that the Earth's centre should be in the same plane with the two stations. For as a rule we must not expect to be able to secure that two convenient stations on the Earth, as well as the centres of the Earth, Venus, and the Sun, should be in the same plane.

Fig. 4.



Dr. De la Rue's remark that by taking a series of pictures the position of Venus may be ascertained at any moment is in reality quite as applicable to my suggestion as to Major Tennant's. In fact, were it not, we might despair of securing the desired object, since we have no reason for believing that astronomers are so certain as to the exact progress of the transit, that the conditions could be secured by anticipatory instructions: whereas by applying Dr. De la Rue's method it will be possible, after the transit is past, to determine with any desired degree of accuracy the position of Venus at the proper instant. And further, it is very obvious that no error in the placing of the fiducial lines for pictures taken at the same stations can much affect the accuracy

of the result, since the comparison of successive pictures, taken at the same station, does not directly involve the element of the solar parallax, as when we have to compare two pictures or paths determined at different stations.

The object, then, of the present paper and the accompanying chart is to determine what stations are most suitable for applying photography to the transit of 1874, on the principles above enumerated. I think the drawing will be found, however, to be also an instructive illustration of the whole character of the transit.

In the last essay but one, I showed how all the chief elements of the transit could be deduced by considering the motion of Venus relatively to a pair of cones, each enveloping the Sun and the Earth, but one having its vertex outside the Earth, the other having its vertex between the Earth and the Sun.

The remaining positions of the Earth in fig. 5, corresponding to the 11 pictures 3–13 in the illustrative plate, are those occupied by the Earth at successive intervals of 15 minutes, the picture numbered 8 corresponding to the position occupied by the Earth at $16^h\ 6^m\ 31^s$ G.M.T., on December 28, 1874, when Venus makes her nearest approach to the centre of the Sun's disc.

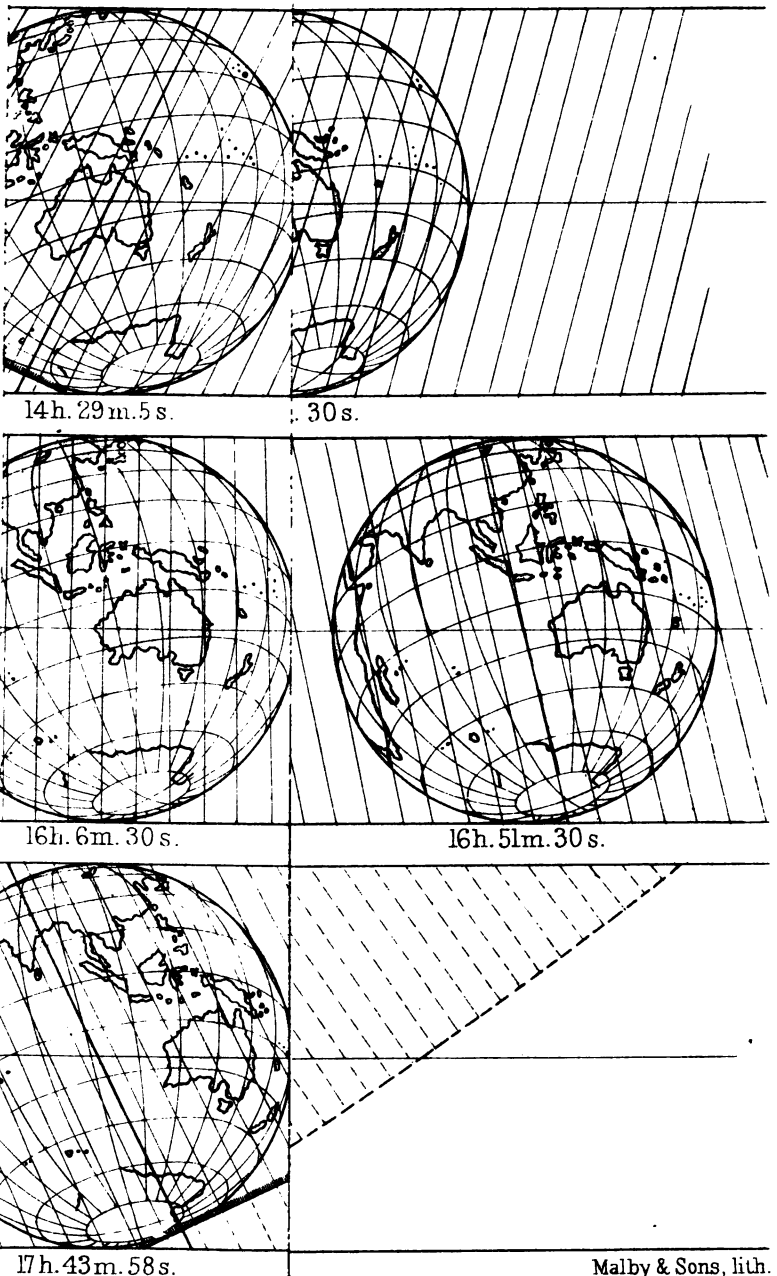
Now if we look at figs. 1 and 2, and consider what they represent, we shall see that fig. 2 may be looked upon as exhibiting an inverted picture of the Sun's disc and the transit of Venus's centre across it: we see, in fact, that the apparent position occupied at any instant by any point on the Earth's surface in fig. 2, corresponds exactly to the position occupied by Venus upon the Sun's disc, as supposed to be seen from that point of the Earth's surface at the instant in question. We have only to invert fig. 2, and look at it from behind, to see what sort of path Venus would seem to traverse upon the Sun's disc, either with reference to the Earth's centre, or to any point of the Earth's surface supposed to be properly depicted upon the small discs 1–15 in fig. 10.

It follows, therefore, that if we want to determine two stations at which at any instant Venus would appear to have a relative parallactic displacement towards the Sun's centre, all that is required is that we select two stations which are on the same radial line from the common centre of the circular sections in fig. 10.

The positions of those radial lines which cross the Earth's track *c d* are exhibited in plate XVII. It will be understood, of course, that the three rows of figures belong in reality to a single row, the numbering of the successive pictures of the Earth indicating the

UGH VENUS

Plate XVII.



14h. 29m. 5s.

30s.

16h. 6m. 30s.

16h. 51m. 30s.

17h. 43m. 58s.

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way in which that row would be formed by the combination of the three rows shown in the plate. The actual arrangement is further illustrated by fig. 5 in which the same numbers are used as in fig. 2.

Fig. 5.

I need not explain the construction of the plate, which depends on the simplest mathematical principles. I have taken a considerable amount of care to secure accuracy, not only in the projections of the Earth, but in the position of the radial cross-lines.

Passing over pictures 1 and 2, we notice in picture 3, that Kerguelen's Land and Crozet Island, lying nearly on a line with certain of the Aleutian Islands, suggest that pictures taken at the former stations at the beginning of the transit could be advantageously compared with pictures simultaneously, or almost simultaneously, taken at a station on one of the easternmost of the Aleutians. In like manner pictures taken near Enderby Land could be advantageously compared with pictures taken at Woahoo. Projection 4 does not differ much from the preceding, but the cross-lines have assumed a less inclined position, and Kerguelen's Land could, at the epoch belonging to this picture, be better combined with a somewhat more westerly Aleutian island.

Projection 5 exhibits the advantage of a photographic station at or near Yokohama. Probably such a station, combined with one in Crozet Island or Kerguelen's Land, would give (by pictures taken near the hour belonging to Projection 5) absolutely the best results which photography can give.

The remaining projections suggest the following combinations of photographic records:—

Projection 6. Yokohama and Enderby Land, Kerguelen's Land and a station in Manchooria; Crozet Island and Pekin; Cape of Good Hope and Nertschinsk.

Projection 7. Kerguelen's Land and Tsitsikar; Crozet Island and Nertschinsk; Cape Town and a station west of Lake Baikal.

Projection 8. Kerguelen's Land and Nertschinsk; Cape Town and Peshawur; Repulse Bay or neighbourhood and Yokohama; Perth (Australia) and Yokohama.

Projection 9. Repulse Bay and Yokohama; Enderby Land and Nertschinsk; Crozet Island and Calcutta; Cape Town and Bombay.

Projection 10. Repulse Bay and Nertchinsk; Possession Island

(near South Victoria Land) and Yokohama; Kerguelen's Land and Calcutta; Crozet Island and Peshawur; Cape Town and Teheran.

Projection 11. Possession Island and Tsitsikar; Repulse Bay and neighbourhood of Lake Baikal; Enderby Land and Calcutta; Kerguelen's Land and Madras; Crozet Island and Peshawur; Cape Town and Aden.

Projection 12. Possession Island and Nertschinsk; Enderby Land and Madras; Kerguelen's Land and Peshawur; Crozet Island and Teheran.

Projection 13. Possession Island and neighbourhood of Lake Baikal; Repulse Bay and Calcutta; a New Zealand Station and Yokohama; Hobart Town and a station near the mouth of the Amoor.

From this list we see that Kerguelen's Land and Crozet Island, Peshawur and other Indian stations, and stations in Siberia, are those which give the most favourable opportunities for the application of the photographic method.

From the *Monthly Notices of the Royal Astronomical Society* for January 1870.

THE DISCUSSION RESUMED.

An important natural phenomenon will occur in the year 1874, and there is some fear that this country—though the Government has been liberal—will suffer serious discredit from the manner in which the phenomenon is to be observed. There is still time, though not a day to spare, to avoid this result; and it is chiefly with the hope of commending the matter to the attention of all who can help to avert national discredit that we submit the facts of the case to general attention, while time still remains for action.

It is known to most of those who read these lines that on December 8, 1874, and again on December 6, 1882, the planet Venus will cross the Sun's face, and that no like phenomenon will occur after 1882 until the year 2004. It chances, moreover, that in one respect the transit of 1874 presents an opportunity which will not recur during the transit of 1882, so that for 130 years astronomers will be without the means of remedying any omission which may be made in the case of the transit now near at hand. It is to this opportunity that what we now have to say specially relates.

Without entering into any scientific details, it may be stated that the importance of a transit of Venus, in an astronomical sense, depends on the fact that as seen from the top or northern parts of the earth, Venus is projected lower down on the Sun's face than as seen from southern stations. And the great object of astronomers when a transit is to take place is to set observers far to the north and far to the south, in order that the observed displacement may be as great as possible. In December, of course, the Arctic regions are turned away from the Sun, so that no observer need be sent there; but the Antarctic regions are then enjoying their nightless summer, and there, if possible, observers should be sent. Moreover, if this is to be done, our country, with its colonies

near the Antarctic seas, is beyond all question the proper country to undertake the task.

Accordingly for many years the question has been discussed. No less than sixteen years ago it was announced that so far as Antarctic voyages were concerned attention might be limited to the transit of 1882. There seemed to be ample time, as well for preparation as for such preliminary reconnaissances as might appear necessary. In 1864 these statements were renewed more positively; and at last, in 1868, geographers and Arctic seamen were invited to give information as to suitable Antarctic stations, or rather as to the accessibility of those Antarctic stations which had been described as astronomically suitable. The geographers and Arctic seamen responded to the appeal. There were gatherings at the rooms of the Astronomical Society and of the Geographical Society; it was announced that Possession Island or Coulman Island, near the precipitous shore-line of Victoria Land, would be a suitable station; the necessary preparations were discussed and almost agreed upon, when,—when it was found that a slight mistake had been made. It was the transit of 1874, not that of 1882, which should have been prepared for by reconnaissances in Antarctic regions. It was shown unmistakably that whatever astronomical observations could be made in 1882 could be made to much greater advantage in 1874; that whereas the Sun would only be five or six degrees above the horizon at the critical moment of the ingress of Venus on the Sun's face in 1882, he will be more than twenty-five degrees above the horizon both at her ingress and egress in 1874; that, in fine, as respects all the essential conditions of the problem, 'some one had blundered.'

A somewhat singular result followed. The author of this correction was almost unknown to the astronomical world (three years before he had been altogether unknown). It was otherwise with the author of the mistake. Ninety-nine persons out of a hundred would have formed but one conclusion on the subject, if the correction had been quietly ignored. This, however, was not what actually took place. A contest (though a feeble one) was maintained over unimportant details; a statement was made that the researches in which the mistakes occurred were only preliminary and rough; the suggested Antarctic voyages dropped out of notice; other mistakes, and especially the complete neglect of certain valuable stations in Northern India, were silently corrected. And most

persons entertained the belief that the author of the correction, having discovered a mare's-nest, wished only to have the discovery forgotten.

Now, at length, however (we may say at the last moment, when the difficulties of Antarctic voyaging are considered, nay, we may almost say, when it is too late), it begins to be recognised that the mistakes pointed out had a very real existence. Every one knows now that Antarctic voyages will not be made in 1882. It is also known that, whether the erroneous preliminary inquiries were only rough first approximations or not, no others have since been made by British astronomers, except those very inquiries by which the errors in question were discovered (and certain corroborative researches published in the 'Nautical Almanac' for 1874); and the inquiry is naturally made if Antarctic voyages were supposed to be worth making for the over-valued transit of 1882, are they to be neglected for the earlier transit now shown to have a greater value even than that of 1882 had been supposed to have? This is a question very seriously affecting the scientific credit of this country. There has been, or let us hope we may still say, there *is* a certain opportunity, in which the whole scientific world has an interest. This opportunity is the only one of the kind since the year 1769, and until the year 2004. To this country specially falls the duty of seizing the opportunity,—*the opportunity, namely, of making absolutely the most effective observations for the determination of the Sun's distance possible during an interval of two hundred and thirty-five years.* What will be said and thought of the science of this country, if, hereafter, it must be recorded that the opportunity was missed through an astronomical blunder, and that, when the blunder was indicated, four precious years were allowed to elapse, during which nothing was done to replace an impracticable scheme by one which could very readily have been carried out? Twelve years of error followed by four years of apathy,—surely if the remembrance of these things can be removed by an energetic effort, the effort is worth making. Let us see what is wanting.

If the work had been begun four years ago, the thing to be done would have been to make reconnaissances for a wintering station near Enderby Land (on the Antarctic Circle, and due south of the Crozets). For that is where the very best astronomical results would be obtained. It is probably too late for this. There remains however, Possession Island, near Victoria Land, in south latitude

72° and east longitude 71°. This chances to be the station which was agreed upon as the best for observing the transit of 1882. It is true that the astronomical authority who had made the mistakes above mentioned had indicated as a suitable station a spot on a precipitous and most dangerous shore-line, where Sir James Ross had not found the slightest sign of an opening. But setting that opinion aside, the geographers and Arctic seamen (amongst them one at least who had accompanied Ross in his Antarctic voyages) agreed that Possession Island was the only available place. It was shown that this station could be readily reached, that a landing could be made (Ross landed a party there in 1842), that with good huts a party could winter there, and that meteorological chances would be favourable. Nothing was said about the Sun's elevation at the critical moments of the ingress and egress of Venus. It was the troublesome person we have spoken of above who first pointed out that the Sun would only be five degrees above the horizon of Possession Island when Venus entered upon his disc. This was a fatal difficulty, because the leading practical authorities had decided that a solar elevation of ten degrees was the very least which would permit of sufficiently accurate observation of the phenomena presented as Venus enters upon the solar disc. Now, as respects the transit of 1874, the Sun's elevation at this very station will be twenty-five degrees (about the elevation of the Sun at noon, in the middle of February in London), when Venus enters on his disc, and thirty-eight degrees when Venus leaves his disc. All the other astronomical circumstances are also most favourable.

The great difficulty is that, owing in the first place to the unfortunate mistake above mentioned, and in the second to the fact that no effort has been made to retrieve matters since the mistake was pointed out, it is now barely possible to get an expedition suitably furnished forth in time to reach Possession Island in January 1874. This is essential, because navigation is not open in Antarctic seas so early as December 8, and accordingly an observing party must winter in Possession Island. Nevertheless, there is still a chance of retrieving matters, if sufficient energy be displayed in good time. It seems hopeless to look for action on the part of the astronomical authority whose influence would be most powerful, since such action would be the admission of mistakes which had been long entertained, and have been followed by an inexplicable apathy. But the facts are now patent; the scientific honour of our

country is at stake; the way to save it is plain and straightforward; difficulties and dangers have not hitherto deterred our countrymen in such matters; and it cannot surely be feared that in so critical a case the mere cost of the required expedition will stand in the way. *If an expedition from this country cannot be managed, one should be sent from Australia or New Zealand.*

From the *Spectator* for February 8, 1873.

THE ASTRONOMER ROYAL'S REPLY.

[*A Letter to the Secretary of the Admiralty on certain Articles which had appeared in the Public Newspapers in regard to the approaching Transit of Venus.*]

'I HAVE the honour to acknowledge your letter of February 14th, calling my attention to articles which have appeared in the "Spectator" newspaper of February 8th, and the "Times" of February 13th, and requesting my views on these for the information of the Lords Commissioners of the Admiralty. I have procured the papers in question, and also the "Times" of February 20th, which contains articles of opposite characters bearing on the same subject; and have also received a paper by Dr. Oppolzer of Vienna, of very elaborate character, scarcely known in this country. I have the honour now to offer my remarks upon the whole subject.

'2. The English papers are moderate and courteous in character, though distinct in their meaning. They are based entirely upon investigations by Mr. R. A. Proctor published in the "Monthly Notices," especially in the "Notice" of June 11th, 1869. These investigations are illustrated by tables and maps of great clearness and unquestionable value; and to these, mainly, I shall refer in the following remarks. I shall, however, also extract some numbers from the calculations made by the Superintendent of the "Nautical Almanac," and published in the "Nautical Almanac" for 1874; and some notes from the paper of Dr. Oppolzer to which I have alluded.

'3. Remarking that the proposal to make observations on the Antarctic continent is essentially connected (as I shall shortly show) with the idea that efficient observations may be made in the extreme north of Asia, I will first point out that this matter entered into my consideration in my original communication to the Royal Astronomical Society in 1857. In speaking of "the application of

the method of difference of duration of transit" (called "Halley's Method" in the newspapers) "to the transit of 1874," I said, "The most northerly stations are to be found in Siberia, Tartary, and Thibet (which will scarcely be visited by astronomers in December), on the coasts of China, and in North British India." And, remarking that the whole discussion is founded on the assumption by Mr. Proctor that comparisons of duration of transit at different places possess a high value, I state that in 1869 M. Puiseux had indicated this; that in the "Monthly Notices," March 12, 1869, I answered M. Puiseux, showing that I had already considered his suggested stations, and also that a ratio of probable errors to which he had not adverted must be taken into consideration. I believe that the idea has not again been promulgated on the Continent. I advert to these historical points for the purpose of showing that the matter has been sufficiently present to my mind. Subsequently Mr. Proctor took up the question, straining to the utmost the idea of separating as far as possible the northern and southern stations, and considering nothing but their geometrical relation.

'4. I may here conveniently cite some passages (translated) from Dr. Oppolzer's paper, page 75: "In Halley's method in general, the longitude of the place of observation needs only approximately to be known, in order to determine the value of the parallax from the time of duration; and this was in the eighteenth century to be regarded as an especially great advantage, as the accurate determination of longitudes was then an almost insoluble problem; but in the present very perfect knowledge of the Moon's motion, there will be no very great difficulty in obtaining satisfactory determinations of the longitude. The application of Halley's method possesses therefore in the present day no special advantage. If the longitude is determined with certainty, it is of more advantage to the accuracy of the result not to use the observations as durations, but to treat them as made for the application of Delisle's method [the comparison of absolute times at different stations]."

'5. My own preparations have been going on entirely in the same spirit as that which influenced Dr. Oppolzer in writing these remarks. The labour which I contemplate as to be employed in determining the local longitudes very far exceeds that for the mere observation of the transit. In a stay of three months I hope to be able to obtain at each station 30 meridional transits of the Moon, and 120 extra-meriodional transits, vertical or horizontal;

and I do not doubt that the longitude obtained will be certain within 1ˢ of time.

'6 Assuming (as Mr. Proctor appears to have assumed, and with my assent thereto) the probable error of clock observation of an ingress or egress as 4ˢ·28, then the probable error of absolute time in seconds is $\surd\{(4{\cdot}28)^2+1\}$,[1] and the probable error in the comparison of absolute times at two stations will be $\surd 2 \times \surd\{(4{\cdot}28)^2+1\}$. This is the probable error for Delisle's method. When both ingress and egress are observed at one station, the probable error of each is 4·28, the probable error of the interval is $\surd 2 \times 4{\cdot}28$, and the probable error of the comparison of the intervals thus observed at two stations is $2 \times 4{\cdot}28$. This is the probable error for Halley's method. The proportion or $\frac{\text{Halley's}}{\text{Delisle's}}$ is 1·379. The demerit of a comparison of observations depends on the proportion

$$\frac{\text{Probable Error.}}{\text{Difference attributable to Parallax}}$$

'Therefore, in order to ascertain whether a comparison in Halley's method is as good as one in Delisle's method, we must find whether the parallax difference in Halley's method is greater than that in Delisle's method in the proportion of 1 : 1·379. And taking Mr. Proctor's selection of a fundamental Delisle's comparison (Woahoo—Crozet Island) at 23ᵐ·8, and remarking that $23{\cdot}8 \times 1{\cdot}379 = 32{\cdot}8$, we finally arrive at this simple criterion, that if the parallactic difference of duration in Halley's method in any special comparison of observations is greater than 32ᵐ·8, that comparison is more valuable than one in Delisle's method; if less than 32ᵐ·8, it is less valuable than one in Delisle's method.

'7. I will now exhibit the application of this criterion to three northern and three southern stations:—

Station	Parallactic Effect m	Comparison with Enderby m	Comparison with Nertschinsk m
Nertschinsk	15·6	35·9	...
Tientsin	13·4	33·7	...
Pekin	12·9	33·2	...
Enderby	20·3	...	35·9
Crozet	16·8	...	32·4
Kerguelen	16·6	...	32·2

[1] In Mr. Proctor's formula, 'Monthly Notices,' 1869, June 11, p. 315, the factor is omitted by a printer's error. The computed numbers appear to be correct.

'The combination of Nertschinsk and Enderby gives a number greater than the criterion; and therefore, if there were no other difficulties in the way, it would be well to combine these two stations. But the combination of Nertschinsk with Crozet or Kerguelen gives a number below the criterion; and therefore, if Enderby is not secured, the observation at Nertschinsk possesses no special value of the kind considered. The combination of Enderby with Tientsin or Pekin gives a number scarcely above the criterion, and therefore, if Nertschinsk is not secured, the observation at Enderby scarcely possesses any special value.

'8. I will now advert to the local circumstances. A few of the following numbers are taken roughly from Mr. Proctor's maps, but none are seriously in error:

Station	Latitude	Sun's Elevation at Ingress and Egress	
	°	°	°
Nertschinsk	51 +	12	10
Tientsin	39	23	22
Pekin	40	22	21
Enderby	(66)	(20)	(40)
Crozet	(46)	(11)	(50)
Kerguelin	49	28	59

'Nertschinsk is a station in Siberia, in high latitude, nearly 1000 miles from the nearest sea. I presume that its climate is truly continental. At St. Petersburg, in the winter, the sun sometimes is not seen for several weeks together; I suppose that the same may happen at Nertschinsk. The Sun's elevation at the observations will be rather small. I doubt greatly the probability that any observations can be made there.

'Of Enderby's Land very little is known. It is certain that an expedition to that region must be exposed to long confinement and great severities, affecting even the metallic instruments.

'9. On a review of the whole case, I decline to recommend that an expedition be sent to Enderby Land, or to any station in the Antarctic continent.

'10. In one of the papers to which my attention was called, it was suggested that, referring to the belief that Lord Lindsay proposes to make observations at the Mauritius, the Rodriguez observations should be abandoned, and a ship thus set at liberty for the South continent. I very highly respect Lord Lindsay's spirit of enterprise, but there are abundant reasons for refusing to abandon a Government expedition in the hope that a private astronomer will

do something equivalent. I add that the position of Mauritius is inferior to that of Rodriguez.

'11. It seems to be conceived by the writers of the papers that no attention has been given to India. For myself I can say that it has been duly considered, but that, in reference to the original proposal of relying on eye-observations, I saw no reason for establishing a station there, and in particular I objected to a proposed substitution of Peshawur for Alexandria. The introduction of photography has introduced new geometrical considerations, principally pointed out by Mr. Proctor; and I have endeavoured to carry out the establishment of a photographic station in India. The matter is now in a stage in which I have no control or official information.

'12. The only extension of the original plan to which I look as a contingency is this. My scheme was drawn up, and action was taken on it, before the outbreak of the war between France and Germany. In proposing that Great Britain should take up a station in the Sandwich Islands, I expressly recorded my hope that France would equip a station at the Marquesas. This hope may now be frustrated, although I know so well the noble spirit of that nation in all matters relating to science that I can scarcely entertain the thought. But I am not aware that any positive arrangement has yet been made, either by the French or the American Governments, for filling up the gap in the Pacific. At present, that is the weak part of established plans. Should the deficiency continue to exist, some effort might well be made to supply it.

'Royal Observatory, Greenwich,
'February 21, 1873.'

From the *Monthly Notices of the Astronomical Society* for March 1873.

REMARKS ON SIR G. AIRY'S LETTER.

I take the articles of this letter *seriatim*.

1 and 2 need no comment other than a recognition on my part of the courteous tone in which they are expressed.

3. I shall presently show that efficient observations at high northern stations are not necessary in order to render observations by Halley's method superior to the proposed observations by Delisle's. I am at a loss to understand how the question can be said to have been sufficiently taken into consideration in the Astronomer Royal's original communication to the Royal Astronomical Society in 1857. I would refer to that communication itself, and to my remarks upon it and quotations from it elsewhere in the present volume. The mere circumstance that the estimated epochs of ingress and egress were nearly an hour in error would suffice of itself to show that that communication did not and could not indicate the actual circumstances of the transit of 1874. It is a mere accident that the places indicated fall on parts of the somewhat extensive regions there named by the Astronomer Royal; but no details as to time-differences or solar elevation could have been deduced from the rough investigation then made, nor as a matter of fact is a single detail of the kind mentioned in that communication. Nevertheless, as it was the first, so also it was the last communication in which the Astronomer Royal made any definite statements respecting the value of Halley's method in 1874. These remarks relate also to the Astronomer Royal's reply to M. Puiseux in the 'Monthly Notices' for March 1869, which I found myself at the time (as I find myself now) unable to reconcile with the paper of 1857. Whether I am mistaken or not in my interpretation of the latter paper can be ascertained by a study of its contents so far as they relate to the transits of 1874 and 1882.

I conceive that a careful comparison of the Astronomer Royal's

discussions of the transit of 1882 with my discussion of the transit of 1874, considering both with special reference to the application of Halley's method, will afford ample evidence that I have not led the way in 'straining to the utmost the idea of separating as far as possible the northern and southern stations, and considering nothing but their geometrical relation.'

4. Premising that Dr. Oppolzer's paper, assuming it to have the Astronomer Royal's assent, has led to a marked change of opinion on his part as to the advantages of Halley's method, I would remark that that paper strengthens all the reasons which have been urged by myself for selecting stations where the whole transit can be observed. For if, besides the use which can be made of the observations of duration at such places, the time observations can be used for Delisle's method also, such stations have an enhanced value. This applies with special force to Antarctic stations for observing the transit, since at every such station advantageous for applying Halley's method, Delisle's method can be applied advantageously *both* at ingress and egress, and *at one or other* more advantageously than at any of the selected southern stations for observations by this method. This remark relates also to stations which are not within the Antarctic circle, and can undoubtedly be reached in December; and it is especially to be noted, that there would be no occasion to make observations for determining the longitude, until the actual observation of the transit had been successfully made. If the weather were unfavourable during the transit, there would be an end of the matter. If the weather were favourable, astronomers could thereafter, at their leisure or convenience, determine the longitude of stations where observations had thus been successfully made. Again the remark applies with special force to the North Indian stations suitable for observing the retarded egress. At Delhi, for example, while the circumstances for observing this special phase are superior (when solar elevation is taken into account) to those at the selected station, Alexandria, the whole transit will also be observable, whereas at Alexandria only the end of the transit will be seen, and that under conditions by no means favourable.

5. This article illustrates strikingly the superior simplicity of Halley's method. Granting Halley's method to be only equal to Delisle's in any given case, it nevertheless must be far less costly if a three months' stay is regarded as necessary for determining the longitude in the application of the latter method. This remark would of course not apply to the suggested occupation of Possession

Island. But it is to be remembered that Antarctic exploration, and particularly an Antarctic wintering expedition, would have very great scientific interest apart from astronomical considerations. In proof of this I need only refer to the passages quoted in the next essay from the Astronomer Royal's communication in the 'Monthly Notices' for May 1865.

6. I must remark that in assuming $4^s{\cdot}28$ as the probable error of clock observation of ingress or egress, I was adopting, without assenting to it, an extreme estimate. I should regard the probable error of such observations made, as during the transit of 1769, to be about $4\frac{1}{4}^s$; but in the observations to be made during the coming transit, guided as they will be by the experience which has been obtained in these matters, I should consider 3^s an ample estimate of the probable error. Adopting this estimate instead of Mr. Stone's estimate (strained to a maximum, I conceive) we obtain a proportion much more favourable to my case.

But *I do not press this point, simply because it is a matter of opinion.* I note only that Mr. Stone's estimate seems to me excessive.

I challenge directly, however, the Astronomer Royal's adoption of my own selection (Woahoo and Crozet Island) as a fundamental Delisle's comparison. I took purposely an extreme case in favour of Delisle's method; and if that extreme case is admitted, so also must the extreme cases for Halley's method. But if, in testing Halley's method, we are to take into account other considerations than geometrical ones, so also must we in testing Delisle's method. I apprehend that the comparison must, in fairness, be made with the Astronomer Royal's proposed arrangements, not with a scheme which was only suggested hypothetically.

7. I will exhibit the application of the Astronomer Royal's criterion (reserving my assent thereto) to his selected stations:

Pairs of Stations	Phase to be Observed	Parallactic Effect m	Time-Difference ×1·379 m
Woahoo . . . / Rodriguez . . .	Ingress	Acc. 11·2 / Ret. 9·9	29·1
Auckland,[1] N.Z. . . / Orsk (Russian Station)	Egress	Acc. 8·5 / Ret. 11·8	28·6
Auckland, N.Z. . . / Alexandria . . .	Egress	Acc. 8·5 / Ret. 10·0	25·5

[1] I find this station in the announced list of places to be occupied by England; but if I remember rightly there has been some change. In default of

It is with these values, and not with the value resulting from the consideration of a station which is not, in point of fact, to be occupied, that comparison should be made. Let it suffice to say on this point that taking the least favourable cases in the Astronomer Royal's table,—viz. Pekin and Kerguelen's Island,—we obtain as the difference of duration

$$12^{m}\cdot9 + 16^{m}\cdot6 = 29^{m}\cdot5$$

which is better than the best of the above values, obtained by applying to the best of the stations actually selected the Astronomer Royal's extreme criterion.

For further evidence on this point see the table which follows this paper.

8. As to local circumstances, I may point out that they affect the application of both the methods. Cloudy weather at Woahoo in December is a contingency far too probable to be overlooked. Observations of Orsk, Omsk, and Tobolsk are not unlikely to be lost through bad weather. Much must inevitably be risked, whatever plans be adopted; and as my suggestions involve a greater spreading of the observers than the Astronomer Royal contemplates (since America, Germany, and France are likely to congregate around the very regions marked out for Great Britain), I conceive that on the whole (and apart from all other considerations) the chances of success would be increased by the suggested change of plan.

I dwell with special force upon the fact that the effective use of Halley's method would render it necessary to occupy two regions, one lying between the northern regions for observing accelerated ingress and retarded egress, the other lying between the southern regions for observing retarded ingress and accelerated egress. This I take to be a most important point; for the chances of a successful result cannot but be reduced if these regions of vantage are left unoccupied.

9. Compare the passages cited from the Astronomer Royal's communications to the Society in 1857, 1864, 1865, and 1868, in my accompanying paper.

10. Relates to a matter respecting which it would be improper for me to express an opinion.

the actually selected station, I adopt the station originally proposed. Whatever difference there may be in the acceleration will be slight. [This is corrected further on.]

11. I venture to quote, in full, from the 'Monthly Notices' for December 1869, the Astronomer Royal's remarks on the stations suitable for observing the egress as retarded by parallax. I cannot find any evidence in these remarks (his last public remarks on the subject) to prove that the North Indian stations suitable for observing this phase (the only phase for which North Indian stations are in question at all) have been 'duly considered':—

'The stations which are favourable for this observation are almost entirely on Russian and Turkish territories. At none of them is the factor less than 0·84; and we have, therefore, only to consider the elevation of the Sun, leaving to the national governments to estimate the facilities or difficulties depending on the locality, the climate, or the season. Any station either to the east or to the west of the Lower Caspian will have the Sun well elevated. Omsk, Orsk (whose longitude has been determined with peculiar care), Astrakhan, Erzeroum, Aleppo, Smyrna, and Alexandria, have the Sun sufficiently high. At Tobolsk, Perm, Kazan, Kharkov, Odessa, Constantinople, and Athens, the Sun will be rather low, and at Moscow it will be on the horizon. We may with the utmost confidence leave the selection of the stations, the determination of longitude, and the observation of the phenomenon, to our Russian friends. One station, however, ought specially to be considered as being, for this purpose, in British hands, namely, Alexandria. It appears not improbable that we may soon have very direct telegraphic communication with Alexandria; but failing this, I trust that no efforts will be wanting to determine accurately its longitude; a longitude which was, in the survey of Admiral Smyth, and which always must be, the zero of longitude in the Levant. This being ascertained, Alexandria would probably be the best of all the stations for observation of the retarded egress.'

It will be observed that India is not even mentioned. I myself only indicated Peshawur as marking a suitable part of India. Delhi and other conveniently accessible stations would serve equally well.

12. Relates to considerations respecting which I have no opinion to offer, beyond the expression of my belief (justified by events which have recently occurred), that the share taken by America in the work of observing the coming transit will be a large and important one.

I may remark, in conclusion, that as respects wintering at

Possession Island, I have no opinion of my own to maintain. I have simply adopted the statements of the most experienced authorities on the subject. When the Astronomer Royal was earnest in endeavouring to secure observations by Halley's method in 1882, he accepted the favourable statements of those authorities readily, or rather, even eagerly; now that Halley's method is being discountenanced, those favourable statements are lightly regarded. For myself I am content, in a matter in which I have not the least experience, to take the opinions of those who are understood to have great experience.[1] If Admirals Richards and Ommanney, and Commander Davis now consider that they were mistaken in what they said in 1868, so be it,—I accept their present opinion. It does not affect my argument. Many stations remain which are certainly accessible early in December, and would give better results by Halley's method than even Sir George Airy expects to obtain by Delisle's.

[1] I pointed out, however, in my 'Sun' (and I believe I was the first to remind the general public of the fact) that Possession Island, owing to the enormous quantities of guano, would be a disagreeable if not dangerous wintering station. Still I supposed that the fact had been in the remembrance of Commander Davis, when he decided that it was possible to winter at Possession Island.

From the *Monthly Notices of the Astronomical Society* for March 1873.

Difference in the Duration of the Transit of Venus in 1874, *at* 14 *Northern and* 17 *Southern Stations; showing the Value of Halley's Method compared with Delisle's (applied at Selected Stations, and judged by Sir G. Airy's criterion.)*

Northern Stations	Southern Stations: Enderby Land	Kemp Island	Possession Island	Adelie Land	Sabrina Land	Crozet Island	Macdonald Island	Kerguelen Land	Emerald Island	Macquarie Land	Campbell Island	Royal Co. Island	Auckland Island	Hobart Town	Canterbury (N.Z.) and Bourbon Island	Melbourne and Mauritius	Adelaide and Rodrigues
	m	m	m	m	m	m	m	m	m	m	m	m	m	m	m	m	m
Nertchinsk . .	35·9	34·2	33·0	33·0	33·0	32·4	32·3	32·2	29·3	29·1	28·7	28·6	28·0	26·0	25·7	24·4	23·9
Taitsikar . . .	34·6	32·9	31·7	31·7	31·7	31·1	31·0	30·9	28·0	27·8	27·4	27·3	26·7	24·7	24·4	23·1	22·6
Kirin-Oula . .	34·4	32·7	31·5	31·5	31·5	30·9	30·8	30·7	27·8	27·6	27·2	27·1	26·5	24·5	24·2	22·9	22·4
Tientsin . . .	33·7	32·0	30·8	30·8	30·8	30·2	30·1	30·0	27·1	26·9	26·5	26·4	25·8	23·8	23·5	22·2	21·5
Jeddo . . .	33·5	31·8	30·6	30·6	30·6	30·0	29·9	29·8	26·9	26·7	26·3	26·2	25·6	23·6	23·3	22·0	21·5
Pekin . . .	33·2	31·5	30·3	30·3	30·3	29·7	29·6	29·5	26·6	26·4	26·0	25·9	25·3	23·3	23·0	21·7	21·2
Tchefoo . . .	32·7	31·0	29·8	29·8	29·8	29·2	29·1	29·0	26·1	25·9	25·5	25·4	24·8	22·8	22·5	21·2	20·7
Nagasaki . . .	32·2	30·5	29·3	29·3	29·3	28·7	28·6	28·5	25·6	25·4	25·0	24·9	24·3	22·3	22·0	20·7	20·2
Bonin Is. . . .	31·7	30·0	28·8	28·8	28·8	28·2	28·1	28·0	25·1	24·9	24·5	24·4	23·8	21·8	21·5	20·2	19·7
Nankin . . .	31·5	29·8	28·6	28·6	28·6	28·0	27·9	27·8	24·9	24·7	24·3	24·2	23·6	21·6	21·3	20·0	19·5
Canton . . .	28·5	26·8	25·6	25·6	25·6	25·0	24·9	24·8	21·9	21·7	21·3	21·2	20·6	18·6	18·3	17·0	16·5
Hongkong . .	28·4	26·7	25·5	25·5	25·5	24·9	24·8	24·7	21·8	21·6	21·2	21·1	20·5	18·5	18·2	16·9	16·4
Peshawur. . .	28·3	26·6	25·4	25·4	25·4	24·8	24·7	24·6	21·7	21·5	21·1	21·0	20·4	18·4	18·1	16·8	16·3
Delhi . . .	27·2	25·5	24·3	24·3	24·3	23·7	23·6	23·5	20·6	20·4	20·0	19·9	19·3	17·3	17·0	15·7	15·2*

NOTE.—The time-difference at Auckland and Alexandria, multiplied by 1·379, amounts to 25m·5; and all values exceeding this in the above Table, that is all above and to the left of the double zigzag, indicate the superiority of Halley's method at the corresponding pair of stations to this selected instance of the application of Delisle's method. In like manner all values above 28m·0, or above and to the left of the single heavy zigzag, indicate the superiority of Halley's method over Delisle's, applied at Auckland and Omsk. All values above 29m·1, or to the left of the single light zigzag, indicate superiority over Delisle's method applied at Wahoo and Rodriguez.

* The maximum observable difference in the transit of 1882 will be 28m (only observable under very unfavourable conditions, and by reaching an Antarctic station at Sabrina Land). It will be observed that the least of the differences in the above Table, 15m·2, exceeds the half of 28m. I venture to point out that, comparing this with the statement in the paper of 1857 that 'the maximum observable difference in 1874 will probably not be half of that in 1882,' I have been justified in regarding that paper as not a sufficiently exact investigation.

THE TRANSIT OF VENUS IN 1874.

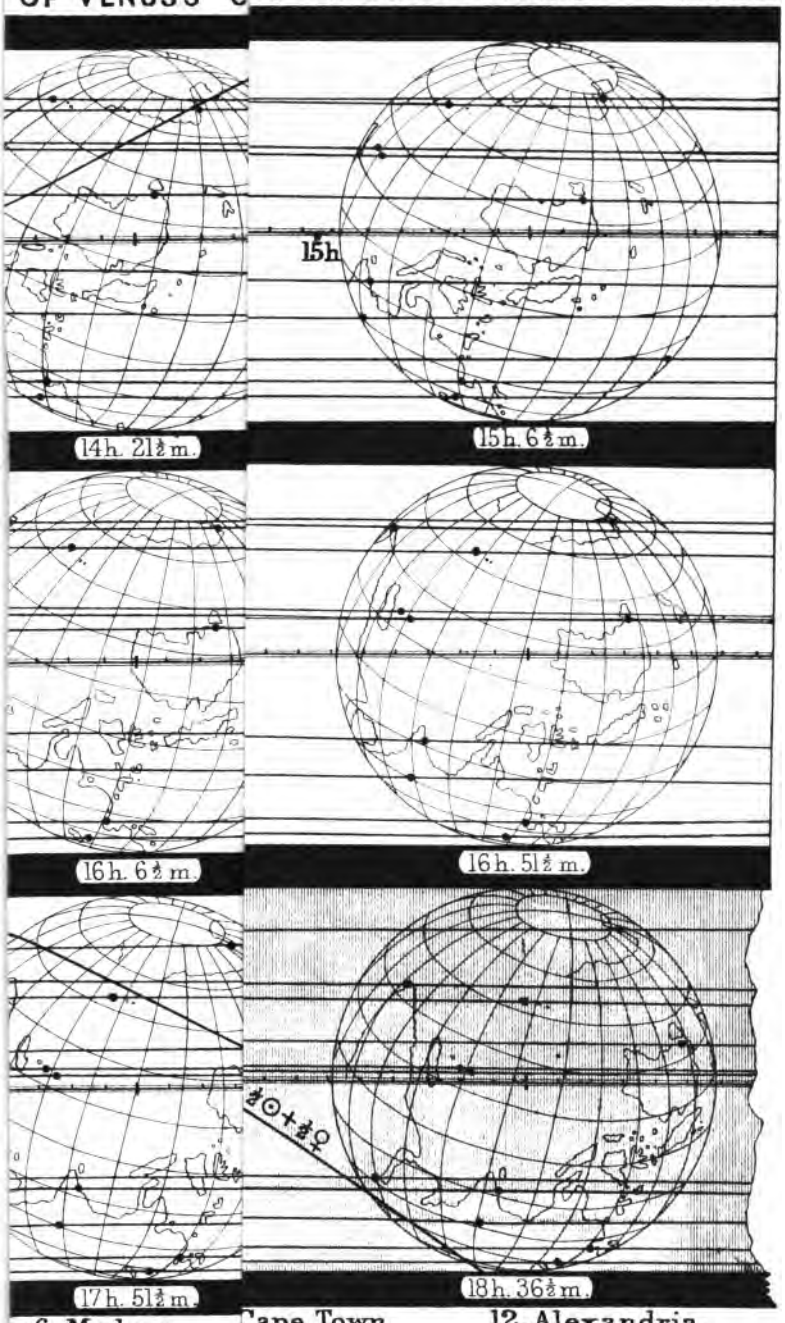
It will be in the remembrance of most of the Fellows of the Astronomical Society that four years ago a paper of mine was read in which the circumstances of the transit of 1874 were subjected to a more careful and detailed examination than had till then (to the best of my knowledge) been applied to the subject. Towards the end of that paper [1] I pointed to certain conclusions respecting the application of Halley's method during the transit, which were certainly new at that time, though they have since been abundantly confirmed in many different quarters, notably in the pages of the 'Nautical Almanac' for 1874. Two months later, or in May 1869, I read a paper in which these conclusions were further advocated, and illustrated by a series of projections.[2] Thereafter, with the exception of two or three short notes to meet objections which had been raised by Mr. Stone to points of detail, I have not, at any of our meetings, or in Council, touched further upon the subject of the approaching transits.

I trust it will not be thought by the Society that I have exhibited any undue impatience. My fear is, indeed, that hereafter the examination of all the circumstances may lead to the impression that I have been remiss (holding the opinions which I entertain on this subject) in leaving the matter to so late an epoch. This,

[1] I feel bound to dwell upon the fact that that paper was commenced, and continued (*pari passu* with the calculations whose results it presents) up to the portion referred to in the title as 'An Addendum,' before I had any notion that the results at which I should arrive would be different from those indicated from the year 1857 to December 1868, by the Astronomer Royal. Anyone who will examine the essay carefully, will recognise abundant evidence of this fact. It was somewhat hastily and quite mistakenly assumed that the paper was 'an attack from beginning to end' on the Astronomer Royal's essays.

[2] In this paper, the series of results which I controverted were simply described as those obtained by a certain process, and they were tabulated under the letter A; Puiseux's results, which were much more near to exactness, were tabulated under the letter B; my own under the letter C.

OF VENUS'S C NG TWELVE STATIONS: Plate XVIII.



6. Madras. Cape Town. 12. Alexandria.

Malby & Sons, lith.

however, will be regarded, I conceive, as a fault on the right side, more particularly when I mention that, as editor of the Proceedings (during Professor Cayley's Presidentship) I have felt some degree of delicacy in touching upon a matter which had formerly been a subject of discussion between myself and the late first Assistant at the Greenwich Observatory.

There is now no time for further delay, at least as respects the part of the subject which I propose chiefly to consider—the advisability, namely, of endeavouring to secure Government assistance for an expedition to the Antarctic regions with the object of applying Halley's method to the transit of December 9, 1874.

Moreover, it is absolutely necessary for the due enforcement of my views (now that time so presses) that I should indicate precisely how and where the mistakes arose which led to the adoption of views altogether different. Because if I fail to do this, the result will inevitably be that many persons will suppose me to be merely advocating a certain opinion (and that against the leading authority in all such matters), whereas in reality the question is one of easily ascertainable facts. In saying this I speak from experience. Four years ago, I published my views without explaining where and how the views which I opposed had had their origin; and I find that quite a large proportion of those who read my papers judged that I merely differed from Sir George Airy on a matter of opinion, not on matters of fact (mathematically testable).

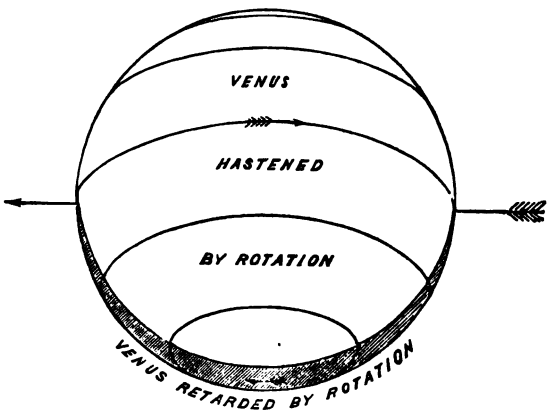
What I propose to prove, in what follows, is, that certain advantageous circumstances, which had been supposed to exist only in the case of the transit of 1882, exist in a greater degree in the case of the earlier transit; that difficulties which render the observations of Halley's method inapplicable in 1882 do not exist in 1874; and, lastly, that the statements (I may say the earnest appeals) made by the Astronomer Royal in the case of the transit of 1882, pledge this country (because made by its official astronomical representative) to the duty of undertaking an expedition to view the transit of 1874 (now that its circumstances are known) from some Antarctic station or stations.

Let it be premised that the reasoning by which, in 1857, it seemed to be demonstrated that Halley's method is inapplicable in 1874, is sound in its general bearing. It breaks down only when tested by details.

Let Fig. 6 represent the face of the Earth as supposed to be

seen from the Sun during a December transit, such as either of the approaching transits. Now, the Earth during the transit is moving from right to left, or in the direction shown by the long arrow. Her rotation shifts points on her surface in the way shown by the small arrows on the latitude parallels, the shift due to this cause being greatest on the equator. This motion manifestly takes place in a sense adverse to that of the Earth's motion of revolution, everywhere except at stations on the shaded lune of the disk. Now, Venus transits with the excess of her motion of revolution over the Earth's; and anything which tends to reduce the effects of the Earth's motion of revolution, increases the excess of Venus's motion—or in other words, hastens Venus in her transit. So that

Fig. 6.



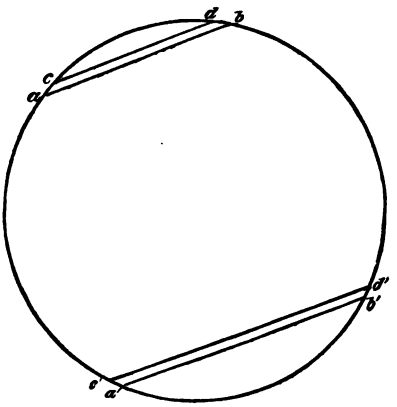
at every point of the unshaded portion of the disk in Fig. 6 Venus is hastened, more or less, by the effects due to the Earth's rotation. On the contrary, at every point on the shaded portion of the disk Venus is retarded in her transit.

Now let it be noticed that these circumstances affect diversely the two transits of such a pair as we are now awaiting. If Fig. 7 represents the Sun's disk, the north point being uppermost, then the lines $a\ b$, $c\ d$, will represent chords of transit in 1874 ($a\ b$ being the chord for a Northern, $c\ d$ being the chord for a Southern observer); and $a'\ b'$, $c'\ d'$ will represent chords of transit in 1882 ($a'\ b'$ being the chord for a Northern, $c'\ d'$ the chord for a Southern station).

Now, it is manifest that in 1874 the conditions affecting the duration of the transit as seen at a Northern station are adverse.

The chord *a b* is longer, owing to the northerly latitude of the observer; but Venus is hastened on her course, and therefore the lengthening is not so great as it otherwise would be. We have then one favourable and one unfavourable condition, the latter to some degree cancelling the former. (In some transits of the kind, the effect of rotation wholly cancels, or even more than cancels, the effect due to latitude.) The Southern station, if taken where, throughout the transit, the observer is on the portion of the disk represented without shading in Fig. 6, will give conspiring effects. The chord of transit *c d* will be shortened, and Venus will be hastened on her course. Hence we have for this station two favourable conditions. In all we have three favourable conditions

Fig. 7.



and one unfavourable condition—so that if the conditions are all equal in value we have a balance of only *two* favourable conditions.

On the other hand, in such a transit as that of 1882 we can theoretically secure four favourable conditions. We have at the Northern station the shortened transit chord *a' b',* and a hastening of Venus, or two conspiring conditions. At a Southern station we have the lengthened transit chord *c' d'*, and by taking a station which throughout the transit lies on the shaded part of the disk (that is, an Antarctic station passing below the pole during the transit hours), we have Venus retarded on her transit path, or again we have two conspiring conditions. In all, then, we have *four* favourable conditions, or twice as many as we obtain for the balance of favourable conditions in 1874.

This is theoretically sound. Moreover, it is quite commonly the case that the effects due to rotation are equivalent to those due to latitude, and that therefore the adverse conditions at a station placed as the Northern station in 1874, may be regarded as cancelling each other. In the celebrated transit of 1769, for example, the conspiring effects of rotation and latitude were nearly equal. The Astronomer Royal, in his 'Popular Astronomy' (published in 1848, be it noticed), justly assigns to rotation 10 minutes out of the observed maximum difference of duration, 22 minutes. It does not seem rash to infer that he had this result in his thoughts (misleading him, in fact) when, after mentioning that the Northern stations best placed as respects latitude would probably not be occupied in 1874, he proceeded to remark in 1857 that the 'observable difference' in the earlier transit would 'probably not be half of that in 1882.'[1] It is at any rate noteworthy that the investigation then made into the conditions of the transit of 1874 did not claim to be accurate on points of detail—in fact, the estimated epochs for the beginning and end of the transit were each of them in error by nearly a full hour. So that we may regard the opinion just quoted as based on the general considerations previously indicated, not on any exact investigations of the circumstances of the transit of 1874 in points of detail. The use of the word 'probably,' in fact, suffices to show this.

But, be this as it may, it is unquestionably the case that those were the last words of the Astronomer Royal on the point in question—'the observable difference of duration' in 1874 'will probably not be half of that in 1882.' When he spoke again about the general subject in 1864, and again in 1865, he confined his remarks altogether to the transit of 1882. In December 1868 he remarked that Halley's method had been shown to 'fail totally' in 1874.

Now let us inquire how far this statement is justified by facts, when details come to be considered.

Let it be noticed that at the Northern station the effect of rotation was supposed to cancel, or nearly so, the effect due to difference of latitude. In other words, at the best Northern station there would be scarcely any lengthening of the durations. This is a very simple issue, relating to a question of fact, not of opinion. Now, in May 1869 (see p. 249), I stated that at Nertchinsk, in Siberia, the transit

[1] See the 'Monthly Notices of the Astronomical Society' for May 1857, p. 215.

would be lengthened by $15\frac{1}{2}$ minutes. How considerable this lengthening is will be seen at once, when I point out that by imagining the Earth's rotation reversed, so as to conspire with the effect of latitude, we could only obtain as an absolute maximum of difference (with the Sun suitably high both at ingress and egress) the value 21 minutes, so that we may regard $17\frac{3}{4}^m$ as the difference due to latitude alone, and capable of being only affected $2\frac{1}{4}^m$ either way by rotation; and it never happens in any transit that the absolute maximum of difference is obtainable. But now as to the accuracy of my statement:—Referring to the 'Nautical Almanac' for 1874, I find the following statements respecting the mean duration of the transit (for internal contacts and the duration at Nertchinsk Mines. At p. 434:

	h	m	s	
Internal Contact at Ingress . . .	14	15	24	G.M.T.
„ „ Egress	17	57	26	
Consequently the duration of the Transit as seen from the Earth's centre is	3	42	2	

At p. 20 of the Appendix:

Nertchinsk Mines

			h	m	
Lat.	51° 18′ N.	Int. Contact at Ingress, Dec. 8,	22	8·2	Local T.
Long.	119 36 E.	„ Egress, „ 9,	2	5·3	
Consequently the duration of the Transit as seen at Nertchinsk Mines is			3	57·1	

Or the lengthening at this station is $3^h\ 57^m\cdot1 - 3^h\ 42^m\ 2^s = 15^m\ 4^s$

Now, if it be noticed that the place dealt with in the 'Nautical Almanac' lies 42′ of latitude south of Nertchinsk itself (at least according to the position given in 'Phillips's Atlas,' Index, p. 22), and that this difference corresponds to about 25 seconds of time in the duration[1] (which is, of course, reduced the further south the station is taken), it will be manifest how closely my graphic construction corresponds with the results of the calculations employed in obtaining the corresponding values in the 'Nautical Almanac.'

And if further confirmation be required let it be noticed that Russia has in a very practical way adopted the conclusion which I believe I was the first to point out, by selecting Nertchinsk Mines as a station for observing the transit. This station is by no means

[1] This will clearly be seen by marking in a point 42′ south of Nertchinsk in Plates XV. and XVI. (which illustrated my paper in the May number of the 'Monthly Notices' for 1869).

good for applying Delisle's method; it is inferior to many Russian stations as respects the accelerated ingress, and to Orsk, Omsk, Tobolsk, Perm, Astrakhan, Odessa, and many other stations, as respects the retarded egress. It is, moreover, a situation which nothing but an amazing zeal in the cause of science could induce any astronomer to select as an observing station in December—since it lies close to the Northern pole of winter cold (it lies, in fact, on the isocheimenal of 13° F. below zero). But because it is the very best Northern station for applying Halley's method, Russia has nobly undertaken to occupy it.[1] This circumstance, apart from the confirmation it affords to the statements which I published four years ago, is one which ought to influence this country strongly, if it should be demonstrated (as I hope to demonstrate a page or two further on), that there are corresponding Southern stations, which this country could occupy, if not less zealous than Russia in the cause of science.

But doubts may still remain whether in stating that 'the observable difference in 1874 will probably not be half of that in 1882,' the Astronomer Royal had the difficulty of finding a Northern station alone in view. Let us, therefore, inquire what is the observable difference, not as between the best Northern station and the mean duration, but as between the best Northern station and some accessible Southern station.

Here, adopting the opinion strongly expressed by the Astronomer Royal (as I shall presently show), that an Antarctic station ought to be occupied if suitable time-differences can thus be secured, we have a very wide choice of places suitable for reconnaissance. But selecting only a station which is known to be accessible, and has been advocated by eminent naval men and geographers (see the 'Monthly Notices' for December 1868, and compare the opinions of Captain Richards, Admiral Ommanney, and Commander Davis), namely, Possession Island, near South Victoria Land, let us compare the difference between the durations at this station and Nertchinsk with the maximum observable difference to be obtained in 1882. Still further to favour the transit of 1882, let us in its case take as the Antarctic station Sabrina Land, though the naval authorities above referred to could find nothing to say in its favour. At this latter station, as compared with the best Northern stations in North

[1] It was soon after known that Russia will occupy *eleven* stations for applying Halley's method.

America, there is, according to the Astronomer Royal's correct estimate, a difference of 28^m very nearly. Now, at Possession Island the transit of 1874 will be shortened 6^m at the beginning, and $11^m{\cdot}4$ at the end, or $17^m{\cdot}4$ in all. Adding this interval to the lengthening by $15^m{\cdot}1$ at Nertchinsk Mines, we obtain a difference of $32^m{\cdot}5$, that is, a *greater* observable difference than in 1882 in the proportion of more than 7 to 6.

But this is far from being all. As a matter of fact, not only is Sabrina Land useless in 1882, because the Sun's elevation at ingress will be only about 4°, but Possession Island (where the difference of duration will be reduced to 24^m) is also useless, because the Sun's elevation will be only 5° at ingress; and there is no other station where Halley's method can be applied at all advantageously in 1882.

Now, in 1874 none of these difficulties present themselves. At Possession Island, which would render available the excellent time-difference of $32\frac{1}{2}$ minutes, the Sun will be 38° high at ingress and 20° high at egress. At Sabrina Land, if this station could be made available, there will be the same time-difference, and solar elevations of 45° and 43° at ingress and egress respectively. At Adélie Land the same time-difference, and the respective solar elevations of 45° and 34°. And lastly, as respects Antarctic stations, at Enderby Land there will be the yet greater time-difference $35\frac{1}{2}$ minutes, and solar elevations 20° and 39° respectively at ingress and egress.

And next, I would invite special attention to the distinct, I may even say the emphatic manner, in which the Astronomer Royal, speaking as the representative of British astronomy, has marked his sense of the duty of this country in the matter of the coming transits. In what I am about to quote he is speaking throughout of the transit of 1882, but the application of his remarks to the earlier transit (now that the superior advantages of Antarctic stations in 1874 has been demonstrated) cannot fail to be generally recognised,

In 1857 the Astronomer Royal's remarks were thus reported ('Monthly Notices' for May 1857, p. 216): 'The southern tract is a part of the Antarctic land discovered by Lieut. Wilkes, of the United States Navy,[1] included between Sabrina Land and

[1] This Antarctic 'land' had, however, been *sailed over* by Ross in 1846–7; and in the later discussion of the subject Sabrina Land was substituted for Wilkes's supposed continent!

event of such an expedition being undertaken, the precise determinations which I have indicated as bearing on the astronomical question must (from the nature of the case) take precedence of all others. But there would be no difficulty in combining with them any other inquiries, of geography, geology, hydrography, magnetism, meteorology, natural history, or any other subject for which the localities are suitable.

'And I have now to request that you will have the kindness to communicate these remarks to the Royal Geographical Society, and to take the sense of the Society on the question, whether it is not desirable, if other scientific bodies should co-operate, that a representation be made by the Royal Geographical Society to Her Majesty's Government on the advantage of making such a reconnaissance of the Southern Continent as I have proposed; primarily in the interest of astronomy (referring to my official responsibility for the importance of the examination at this special time); but conjointly with that, in the interests, perhaps ultimately more important, of geography and other sciences usually promoted by the Royal Geographical Society.'

I need scarcely remind my readers of the paper read before the Society by the Astronomer Royal in December 1868. Nevertheless, it is necessary, first, to point out that at that late epoch the error respecting the transit of 1874 still remained uncorrected, and that the Astronomer Royal then (see p. 33 of the 'Monthly Notices' for December 1868) repeated that 'the method by observation of the interval in time between ingress and egress at each of the stations at least, on nearly opposite parts of the Earth, fails totally for the transit of 1874.' At that time also, notwithstanding the relatively unfavourable circumstances for applying this method (Halley's) to the transit of 1882, and the very favourable conditions under which Delisle's method can be applied in 1882, he urged that only three stations should be occupied for Delisle's method in that year, the instruments of the five 1874 expeditions, 'thus set free from two stations,' being required at an observing station on the Southern continent. He had now so far changed his mind as to the method of dealing with Antarctic difficulties, as to speak in the following terms:—'The choice of station being made,' he said, 'I would not recommend any reconnaissance, but I would propose that an expedition should go direct to the selected point in good time for the observation of the phenomenon. The season is early for South Polar expeditions, and any difficulties produced by ice would

probably diminish every day. A station being gained, all that is necessary in the way of subsidiary observation is, a few days' observation to give clock-rate; then the clock times of the two phenomena will furnish all that is required. The first action to be undertaken by the Government,' he proceeds (and I invite special attention to the point), 'is to procure the stock of instruments, and this ought to be done without delay. An observing plant like that' (described in the earlier part of the same paper) 'is not to be obtained in haste, and the proposed expedition might be entirely crippled by a small negligence on this point. The equipment of ships and the selection of officers would probably require much less time.'

It will be noticed that if such a plan as this could be followed out in 1874, the necessity of wintering in Possession Island would be avoided. However, it appeared to the naval authorities who followed the Astronomer Royal in addressing the meeting, that the more certain course for achieving the desired result would consist in the preparation of an expedition to winter in Possession Island. I quote the following passages as bearing specially on the feasibility of such an expedition:—

Admiral (then Captain) Richards, Hydrographer to the Admiralty, said: 'My own opinion, looking to the uncertainty of finding a wintering station for a ship, is that landing a party on Possession Island,' or one of the islands farther south, 'would be the most feasible course, and there would be little doubt of the facility of reaching one or other of these islands with a suitable steam-vessel, making Tasmania or New Zealand the base of operations. Doubtless a year passed in this region would be most profitably employed in adding to our knowledge of magnetism, and various other branches of physical science.'

Admiral Ommanney said, *inter alia*: 'I fully concur in all that has fallen from the Hydrographer to the Navy, and hope ere long to hear that operations are making for sending out to explore the Antarctic Seas.'

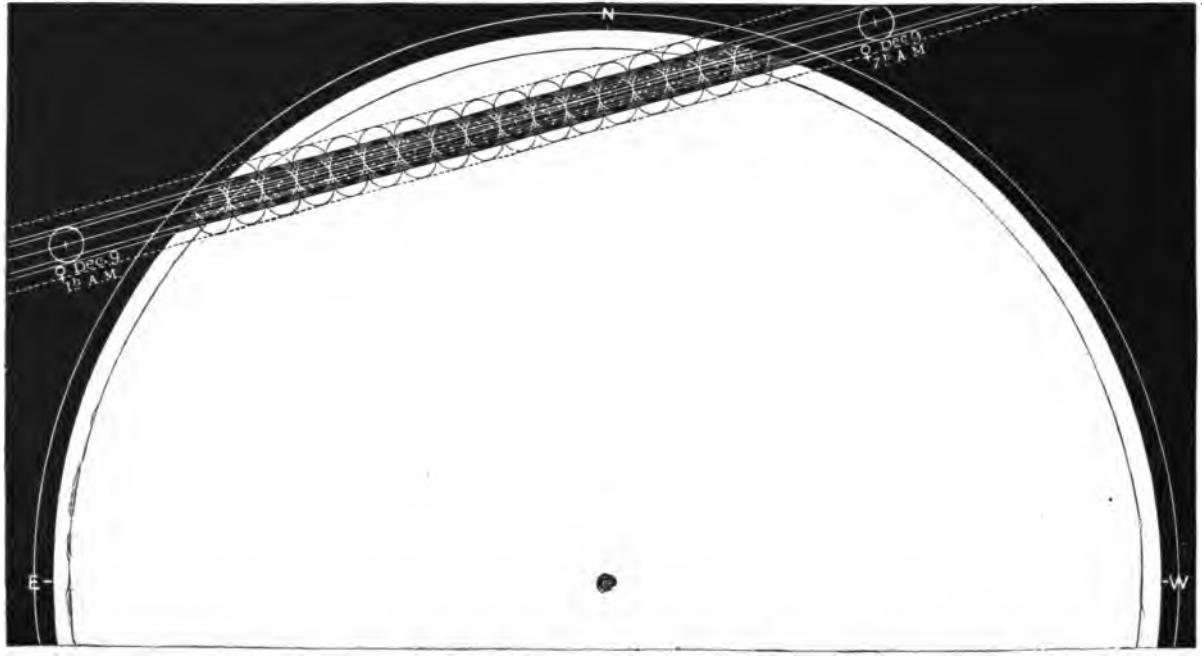
Commander J. A. Davis, who had accompanied Sir James Ross in that most gallant expedition during which Victoria Land was discovered, and who had himself landed at Possession Island, said that 'he believed there would be no difficulty whatever in again effecting a landing in the same place.' 'With regard to the period of the season at which the transit took place, it was to be remembered that the 6th of December was so early that no ships

had ever reached the Antarctic Circle by that date; and as it would be necessary to arrange the instruments, &c. preparatory to the observation, he might say that the ships ought to be on the spot at least a month before. This would be the 6th of November, a date altogether out of the question; and as the ships could not winter in the South, the party would necessarily have to land the year before; but with good tents he had no doubt they could pass the winter very comfortably' (this, of course, and what follows, will not be taken strictly *au pied de la lettre*): 'they would have a pleasant prospect before them, and plenty of penguins to live on. In comparison with Kerguelen Island and the Crozets,' he proceeded, 'the chances of observing the transit—meteorologically speaking—would be greatly in favour of South Victoria.'

Captain Toynbee also expressed an opinion strongly adverse to the meteorological chances at Prince Edward's Islands, the Crozets, and Kerguelen Land, since their neighbourhood is, he said, 'so far as my experience goes, subject to a great deal of thick weather.'

It remains only to mention that Plate XVIII. which illustrates this paper, besides being useful in showing the path of Venus's centre across the Sun's disk, as seen from the stations named, and indicating the corresponding path for any station whatever, affords an independent proof of that which, however, has already been abundantly demonstrated—the fact, namely, that Halley's method is most advantageously applicable in 1874. The chart requires little explanation. The simplest geometrical considerations will show that, imagining a long line to extend through Venus's centre at any moment during transit to the Earth on one side, and the Sun on the other; then if the end towards the Earth be supposed to be carried swiftly along the outlines of the terrestrial continents, and over the meridians and parallels, the end towards the Sun would trace out such projections as are shown in the chart. Moreover, it is manifest that from whatever point on the Earth such a line extends, the point in which the line meets the Sun is that on which Venus's centre is at the instant projected. Accordingly, we have only to determine the aspect of the Earth's disk as seen from the Sun at any instant, and the position of Venus's centre, on the Sun's disk, as seen from the Earth's centre at that instant, to have at once the means, by constructing such an inverted projection as is seen in the successive pictures of the chart, of determining the apparent position of Venus's

Plate XIX.



Illustrating the transit of *Venus* across the Sun's Disk, on December 9, 1874.

centre as seen at that instant from any point of the Earth's sunlit hemisphere.

The chart itself shows clearly the relation between the strip of the Sun's disk (divided into three portions in the chart) and the outline of that disk. Moreover, the circles marked $\frac{1}{2}\odot+\frac{1}{2}\varphi$, and $\frac{1}{2}\odot-\frac{1}{2}\varphi$ indicate by their intersection with the various transit chords, where external contact and internal contact respectively take place; for, manifestly, when Venus's centre, as seen from any station, is on the circle marked $\frac{1}{2}\odot+\frac{1}{2}\varphi$ (that is, is at a distance from the Sun's centre equal to the sum of his radius and Venus's) Venus must be at the moment, and as seen from that station, in external contact; and similarly she must be in internal contact when her centre, as seen from any station, is on the circle marked $\frac{1}{2}\odot-\frac{1}{2}\varphi$.

But Plate XIX. serves to show more clearly how the illustrative chart is to be interpreted. It shows the northern half of the Sun's disk, and indicates the relative dimensions of the disk of Venus and the Sun, as well as the maximum parallactic displacement of Venus.

I apprehend that it has been demonstrated that (i.) the Astronomer Royal's first and only discussion of the suitability of Halley's method in 1874 was based on insufficient evidence, was in itself incomplete, and led him to an erroneous opinion; (ii) that not only is the method more advantageously applicable in 1874 than in 1882, as regards time-difference, but that the objection of low solar altitude at a critical phase in 1882 has no existence in 1874; (iii) that the Astronomer Royal himself warmly advocated the equipment of Antarctic expeditions for viewing the transit of 1882 by Halley's method, notwithstanding the known difficulties; and (iv) that the best naval authorities on this special subject concur in regarding Antarctic expeditions for viewing a transit early in December as altogether practicable.

The conclusion directly deducible from these results cannot be mistaken. England's duty is more than manifest; it has been to all intents and purposes admitted by her astronomical and nautical official representatives. And I cannot but express my conviction that it will be little less than a national calamity, as assuredly it will be scientifically most regrettable if any considerations, either of convenience or of personal dignity on the one hand, or of false courtesy on the other, should lead to the loss of opportunities which will not be again available for many years to come.

From the *Monthly Notices of the Astronomical Society* for March 1873.

THE DIRECT METHOD OF OBSERVING TRANSITS.

VERY early during my examination of the subject of the approaching transits, I was led to adopt and state the opinion that the parallactic displacement of Venus and thence the Sun's parallax, might, in the present state of instrumental astronomy, be determined at least as accurately by direct measurement of Venus's position at successive epochs of her transit as by either Delisle's or Halley's method. It appears impossible to eliminate the error resulting from the clinging of Venus to the Sun's limb, after ingress and before egress; and although several contrivances have been suggested for reducing this error, it is doubtful whether any of them will prove successful. It remains to be shown, moreover, whether photography can be successfully applied to determine the parallactic displacement of Venus.

I find that the German astronomers have for some time recognised the advantages which would probably result from such processes of measurement as I have mentioned; and their selection of Tchefoo, where the whole transit will be observable, indicates in a marked manner their preference for the direct method, since Tchefoo is not an exceptionally advantageous station for observing the accelerated ingress, and still less for observing the retarded egress. It is also inferior to other Northern stations for applying Halley's method; and indeed German astronomers have definitely indicated their preference for the direct method.

The American astronomers have also adopted a favourable opinion as to the direct method.

It appears to me that if English astronomers are to base their methods of procedure on foreign opinions (a growing fashion which I myself am far from urging as desirable), attention might not disadvantageously be directed to the considerations resulting from the above-mentioned opinions of German and American astronomers. It is easy to perceive what these considerations are.

In the first place, it is manifest that, *cæteris paribus*, those

stations will be most advantageous which show the whole transit under the most favourable conditions; and in comparing these stations, we should regard that station as the better which shows the greater proportion of the transit favourably. So that, so far as this method of observation is concerned, our selected stations at Woahoo and Alexandria would be altogether inferior to such a station as Tchefoo, since at Woahoo the Sun sets before half the transit has taken place, and at Alexandria more than half the transit is already over when the Sun rises.

It is probable, however, that few would be disposed to sacrifice such a station as Woahoo, where Delisle's method is applicable under conditions exceptionally advantageous. But it may be worth while to inquire whether so much can be said in favour of Alexandria: in fact, it appears to me that but one answer can be made to this question, so far as it relates to the action called for on England's part.

To begin with, it seems not wholly unreasonable to expect that either Italy or Greece should occupy the only really advantageous Mediterranean station. Moreover, France is to occupy Suez, and the circumstances of the transit at Suez and Alexandria will be very nearly identical, as can be seen at once from Plate XVIII. which illustrates the present essay.

But it is when we compare the circumstances of the transit at Alexandria with those which will be presented at North Indian stations, such as Peshawur, Delhi, and so on, that we find most occasion to regret the unfortunate accident by which these North Indian stations came to be so long and so completely overlooked.

As respects the application of Delisle's method, the advantage of Peshawur is sufficiently marked. The retardation is indeed but a third of a minute greater at Peshawur than at Alexandria. But the Sun is unfortunately very low at Alexandria—only 14° above the horizon—when egress occurs, whereas at Peshawur the Sun will be more than 31° above the horizon.

The main point, however,—and I conceive it to be an extremely important point—is that at Peshawur, at Delhi, and at many stations over the region between these places, the whole transit will be visible, and therefore processes of direct measurement can be most effectively applied. When we add to these considerations the circumstance that it is much more manifestly the duty of Great Britain to occupy this advantageous region in her own territory than to occupy Alexandria, but one opinion can be formed.

I dwelt on these considerations, some nine months since, in a letter addressed to the Astronomer Royal. He pointed out that it was easier (and more advantageous to geography) to determine the latitude of Alexandria than that of Peshawur, and moreover that the position of Peshawur, near the Khyber Pass, was altogether unfavourable. Knowing something, however, of the zeal with which the survey of India is being prosecuted, and of the importance of several exact determinations of the longitudes of Indian stations, I am not altogether convinced that the determination of the longitude of Peshawur would present insurmountable difficulties or be utterly useless in a geographical sense. I observe also that the Indian railway system is to be extended to Peshawur, and therefore I venture to surmise that the place is not altogether inaccessible.

I note, in conclusion, that stations useful for Halley's method are (in the present instance) always useful, and sometimes among the very best stations for Delisle's method, while they are manifestly the most advantageous stations for the direct method. The inference is too obvious to need enforcing.

From the *Monthly Notices of the Astronomical Society* for March 1873.

RISK OF ABSOLUTE FAILURE.

I OBSERVE that, in Mr. Penrose's valuable contribution to the discussion of this subject, he overlooks certain considerations relative to Southern stations, which are I think extremely important. In fact, none of the papers on the transit which I have hitherto seen dwell with adequate force on the circumstance to which I am about to invite attention.

It does not seem to be noticed that there is great risk of the whole series of observations of the transit being rendered useless for want of an adequate number of Southern stations.

At Kerguelen's Island bad weather is more than likely; it is all but certain, as any one can ascertain by studying Sir J. C. Ross's narrative of his Southern voyages. The same remark applies to Crozet Island, which, however, has not as yet been selected by any country. Observations in Tasmania and South Australia will not be of great value. There remain then only Rodriguez, Mauritius, and Bourbon, for retarded ingress, and the New Zealand stations for accelerated egress. If bad weather prevails over the group of islands first named, and also in New Zealand, it is all but certain that the whole affair will end in complete failure. Be it noticed also, that good weather in either region would only avail for Delisle's method, and would be insufficient if bad weather prevailed at but one region of Northern stations

Let the chances, as matters are at present arranged, be but carefully weighed, and I feel assured it will be recognised that there is very great occasion for anxiety as to the result. Northern chances outweigh Southern chances ten to one, but the balance counts for nothing. Success depends altogether on observations being made in both hemispheres.

If, however, we consider only the chances relating to three out of the four methods on which astronomers as a body place reliance, the matter assumes a much more serious aspect. The four methods

are Delisle's, Halley's, the photographic method, and the direct method. Take now the last three. For the application of these methods the Russians have made ample provision, so have the American astronomers, and the Germans will occupy at least one station, Tchefoo, specially for these methods. Every preparation is being made, in fact, for Northern work (except only that our North Indian region, available for these methods as well as Delisle's, is not sufficiently provided for). But now what is there to balance all this, in the Southern hemisphere? *Of really first-class stations there are but three which have even been mentioned—viz., Crozet Island, Macdonald Island, and Kerguelen Island. Of these only Kerguelen Island has been actually selected; and here bad weather is almost a certainty. Of the other stations,—Canterbury (N.Z.), Chatham Island, Bourbon, Mauritius, and Rodriguez,—it is only necessary to remark that they are very inferior for these three important methods.*

It is on this account chiefly that I have been earnest in my appeal for the occupation of Antarctic and sub-Antarctic stations. If anything were required to add to my anxiety on this subject, it would be found in the manifest reliance placed by Russia, America, and Germany on the methods in question.

I am concerned to think that reconnaissances over the regions between Kerguelen Island, Enderby Land, Possession Island, and Auckland Island, may be absolutely necessary for a proper choice of stations; that such reconnaissances might have been made since I first dwelt on these matters four years ago; and that possibly had I been earnest in advocating these considerations during the last four years, either Great Britain or America might before this have found suitable observing stations in the above-named region. I judged it best simply to indicate the state of the case and wait. I fear I may have been mistaken, though it is difficult to see what could have been done until the approach of the event itself and the declared intentions of other countries enforced attention to the circumstances I have touched upon. I trust it may still not be too late to provide for an adequate number of Southern stations sufficiently far apart to give proper chances of success. I do not hesitate to say that in my opinion the provision hitherto made is altogether inadequate, so far as Southern stations are concerned.

From the *Monthly Notices of the Astronomical Society* for April 1873.

APPEAL TO AMERICA.

[*A note addressed to the Astronomers of the United States, on the subject of the approaching Transit of Venus, at the suggestion of a distinguished European Astronomer.*]

DURING the course of correspondence which I have had on the subject of the approaching transit of *Venus* with one of the most eminent astronomers of this or any country, the idea has been suggested to me by him that advantage would result to science if an appeal were made to America to furnish forth expeditions to the Antarctic and sub-Antarctic regions for the purpose of making those southern observations without which the northern observations at Halleyan stations will be altogether useless. 'America has frequently shown great interest,' he remarks, 'in Southern exploration, while she also possesses good telescopes and competent observers to use them. Let both countries do their best, and science, which is of no country, will benefit all the more.'

I gladly act upon this suggestion, remarking only that while I recognise the abstract justice of the proposition that science is of no country, I cannot altogether free myself from the hope which I have long entertained and expressed, that in the struggle to advance scientific knowledge this country may worthily maintain her position.

My appeal to America is based on considerations which I have already urged elsewhere. If the great problem for which the coming transits are to be observed is really important to science (on which point no one, I suppose, can entertain any question), then the circumstances to which I advert are of no light significance.

We need not closely inquire whether one interpretation or another of the peculiarities of internal contact be correct. It is not a question whether one or another method have some slight or

even considerable advantage. Nor again is it a question whether this or that Antarctic or sub-Antarctic station can be occupied or not.

What I urge on our American fellow-students of astronomy, as I have urged and still urge at home, is the adoption of arrangements for occupying *many* stations in the Southern hemisphere, lest the whole matter end in failure, or in a success so partial as to compare very unfavourably with what was accomplished in 1769. I mention as a mere detail, that the distinguished astronomer whose advice I am following, altogether concurs in my opinion that the *duration* of the transit should be observed at as many favourably situated Southern stations as possible. And every one who considers what Russia, America, and Germany are preparing to do at northern stations,—no less than sixteen of which are to be occupied where durations can be favourably observed,—must feel how necessary it is to call attention to the fact that at present there are but four or five third-class Southern stations for observing durations, and only one first-class station. But though I cannot but dwell on this fact, fortified as it is by the circumstance that the photographic and direct methods are equally ill provided for, I do not rest my appeal on details of the sort. It is the risk of absolute failure, and the certainty that the Southern stations hitherto provided for are insufficient in number, to which I earnestly invite the attention of American astronomers.[1]

The region to be occupied is indicated in the chart which appears in the present monthly number. Of Antarctic stations there are Enderby Land, Sabrina Land, Adélie Land, and Possession Island, as well as the whole region (including these places) surveyed by Wilkes, Ross, D'Urville, Billingshausen, and others. Of sub-Antarctic stations there are Kemp Island, Macdonald Island, Emerald Island, the Crozets, Royal Co. Island, and others, very uninviting beyond all question, but doubtless including several accessible stations. Unfortunately there is now little time for preliminary survey during the Antarctic summer of 1873–74; but if such survey cannot be undertaken, then in the autumn (or Antarctic spring) of 1874, two or three ships (preferably steam ships)

[1] Granting even that fine weather prevailed at each of the few Southern stations, the probable error of the resulting determination of the solar parallax must necessarily be enhanced when the Southern stations are so few compared with the Northern.

might proceed direct to the region indicated, each conveying two or three well-provided observing-parties, and combining reconnaissance with the occupation of stations as they were successively selected.

That it is perfectly in the power of this country and America to ensure the requisite number of Southern observations of the coming transit, I am satisfied. There is, it is true, no time for delay. Energy and skill will be wanted; but they have never been looked for in vain in such circumstances. The expeditions which would have to be made would be no pleasure-parties, nor would they be free from difficulties and dangers sufficient to tax the courage even of British and American seamen. But these very considerations encourage the students of science in both countries to believe that the required effort will be made. That it should be made, if failure is to be averted, does not seem to me to be open to the slightest question.

From the *Monthly Notices of the Astronomical Society* for May 1873.

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ON A STEREOGRAPHIC CHART OF THE TRANSIT OF 1874.

ACTION OF THE GREENWICH BOARD OF VISITORS.

At the Visitation of the Greenwich Observatory on June 7, 1873, *it was proposed by Professor Adams, and carried unanimously, that Government should be applied to for the means of organising parties of observers in the Southern Ocean, with the view of finding additional localities in the sub-Antarctic regions for applying Halley's method to the observation of the transit of* 1874.

PLATE XX. is intended to illustrate a relation to which Professor Adams called attention at the last meeting of this Society. If we disregard the rotation of the Earth during ingress or egress, and also neglect the curvature of Venus's shadow-cone where it crosses the Earth, it is manifest that the points where the shadow touches the Earth first and last, at ingress or at egress, are respectively antipodal, and may be regarded as the poles of a series of circles of equal acceleration and retardation, of equal value therefore for applying Delisle's method. Moreover, these circles manifestly indicate a value proportional directly to their distance from the plane of the great circle having the before-mentioned points as poles. The intersections of these circles indicate points of a particular value for Halley's method, the excess or defect of duration being (i) the sum of the corresponding accelerations or retardations where each of two intersecting circles indicates a time-difference of the same kind, or (ii) the excess of acceleration over retardation, or of retardation over acceleration where the time-differences are of different kinds. It is readily seen that if points of equal value for Halley's method are connected, the connecting curves are a series of circles, having as poles the points midway between the poles of maximum retardation and those of maximum acceleration. Moreover, these circles, like those of equal value for Delisle's method,

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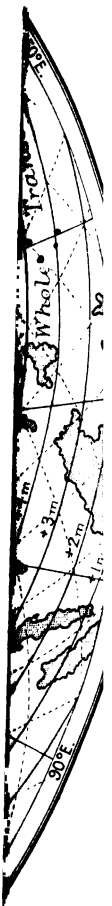

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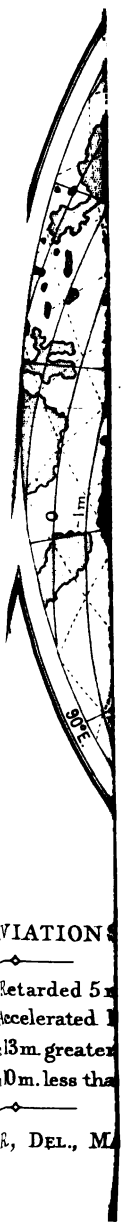

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indicate values directly proportional to their distance from the plane of the great circle having these mid-points as poles.

In my chart the several curves corresponding to these circles have been drawn; but *all the corrections depending on the Earth's rotation during ingress and egress, and on the curvature of the shadow-cone, have been carefully taken into account.*

The dotted red[1] curves are those indicating the *loci* of points of equal value for Delisle's method, and the red curves indicate the *loci* of points of equal value for Halley's method. The actual accelerations or retardations in minutes, and the differences of duration, have been indicated in red letters.

The interpretation of the chart, and the regions indicated as suitable for the various methods proposed to be employed, will be manifest even on a very slight inspection of the chart.

[1] As a rule, I prefer to have only one printing in a chart of this sort, where every line has been laid down with scrupulous accuracy, because the coloured lines may not be printed quite correctly. But in the present case, the map would have been overcrowded by black lines, unless red had been used. To learn the amount of error in 'registering,' it is only necessary to compare the red and black impressions of the small cross lines indicating the points of maximum acceleration and retardation.

From the *Monthly Notices of the Astronomical Society* for May 1873.

de la parallaxe solaire, par les observations de contact, c'est-à-dire par la méthode qui, après tout, donnera probablement les meilleures résultats. Il est donc à désirer que rien ne soit négligé pour assurer dans les meilleures conditions l'observation du prochain passage. . . . On pourra, en effet, sans sortir des régions facilement accessibles, obtenir en 1874 des différences de durée de passage s'élévant à 26 minutes, des différences d'heures d'entrée de 21 minutes, et des différences d'heures de sortir de 18 minutes, tandis qu'en 1882 ces différences se réduiront la première à 16 minutes, et les deux autres à 15 minutes.'

A brief study of Plate XXI., and a comparison between this plate and Plate XX. illustrating the transit of 1874, will suffice not only to confirm these statements (and my own statements to the same effect in 1871), but to show on what circumstances the superiority of 1874 over 1882 for Halley's method depends. I may remark, indeed, that the superiority of 1874 for Delisle's method is more apparent than real, being to a great extent (Mr. Stone thinks wholly) counterbalanced by the slowness with which Venus crosses the Sun's limb in 1874.

I would invite special attention to the position of the Halleyan curve marked o in the two maps, which curve may be called the Halleyan equator, since it marks the curve on the Earth where the duration has its mean value. It will be seen that this curve lies much farther south for 1882 than for 1874. It leaves a very limited region outside the Antarctic circle, and if we take lines 10° from the curves marking where transit begins and ends at sunrise and sunset, these lines being taken within the region where the whole transit is seen, it is found that the region of the Earth where the duration will be less than the mean, with a Sun not less than 10° above the horizon both at ingress and egress, is very limited indeed. But in 1874, on the *unfavourable* or northern side of the Halleyan equator, we can find places where an excess of duration of more than 15 minutes, with the required conditions as to altitude, can be obtained.

If we assume, in fact (which I think will be generally admitted), that no station can be regarded as suitable for Halley's method where the difference between the actual duration and the mean duration is less than half the maximum acceleration or retardation, or where the Sun is less than 10 degrees high at ingress or egress, then absolutely no station whatever is available in 1882, unless the South pole can be approached much nearer even than it was

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approached by Sir Jas. C. Ross in the famous expedition when Possession Island was discovered.

I confess that the prospect of successful observation at Possession Island, with a Sun only 5° high at ingress, seems to me so slight that I should hear with regret of any attempt to carry out the suggested scheme for wintering at Possession Island in 1882.

From the *Monthly Notices of the Astronomical Society* for June 1873.

A NEW METHOD OF OBSERVING THE TRANSITS OF VENUS.

Mr. E. L. Garbett has communicated to me his views respecting a method of observing the approaching transits of Venus, which appears to offer considerable advantages. He suggests the application of photography with special reference to the middle of the transit—that is, that stations should be selected where at the middle of the transit Venus will be most displaced by parallax from and towards the Sun's centre. This differs from Dr. De la Rue's original proposition, in which stress was laid, if I remember rightly, on the determination of the distance of Venus at mid-transit from the Sun's centre by the comparison of photographs taken during the whole progress of the transit. What Mr. Garbett proposes is that attention should be directed solely to the determination of the distance of Venus from the Sun's centre at the time of mid-transit by several photographs taken during a brief interval including that epoch.

The best available station for the purpose, in a geometrical sense, would be Bouvet Isle, south and somewhat west of Cape Town. But Cape Town would be an excellent station; and I cannot but express a hope that the necessary photographic appliances for this method will be provided there, in addition to those which can be favourably applied at that station for indicating the whole progress of the latter half of the transit. As our excellent late Secretary, Mr. Stone, is in command there, we may be sure that the fullest and most satisfactory use would be made of any appliances so provided.

From the *Monthly Notices of the Astronomical Society* for June 1873.

ON THE SOUTHERN REGION IN WHICH RECONNAISSANCE SHOULD HAVE BEEN MADE BETWEEN 1869 *AND* 1874, *TO FIND WHAT STATIONS, IF ANY, ARE CONVENIENT AND SUITABLE FOR OBSERVING THE TRANSIT OF VENUS IN* 1874.

To complete the processes of charting which I have thought it desirable and necessary to undertake in connexion with the transit of 1874, I now present in Plate XXII. a chart of the regions where the duration of the transit will be considerably shortened. It will be observed that the map includes every point on the Earth's surface where the duration of the transit will be less than the mean duration by eight minutes, the Sun not being less than ten degrees high both at ingress and egress (internal contacts).

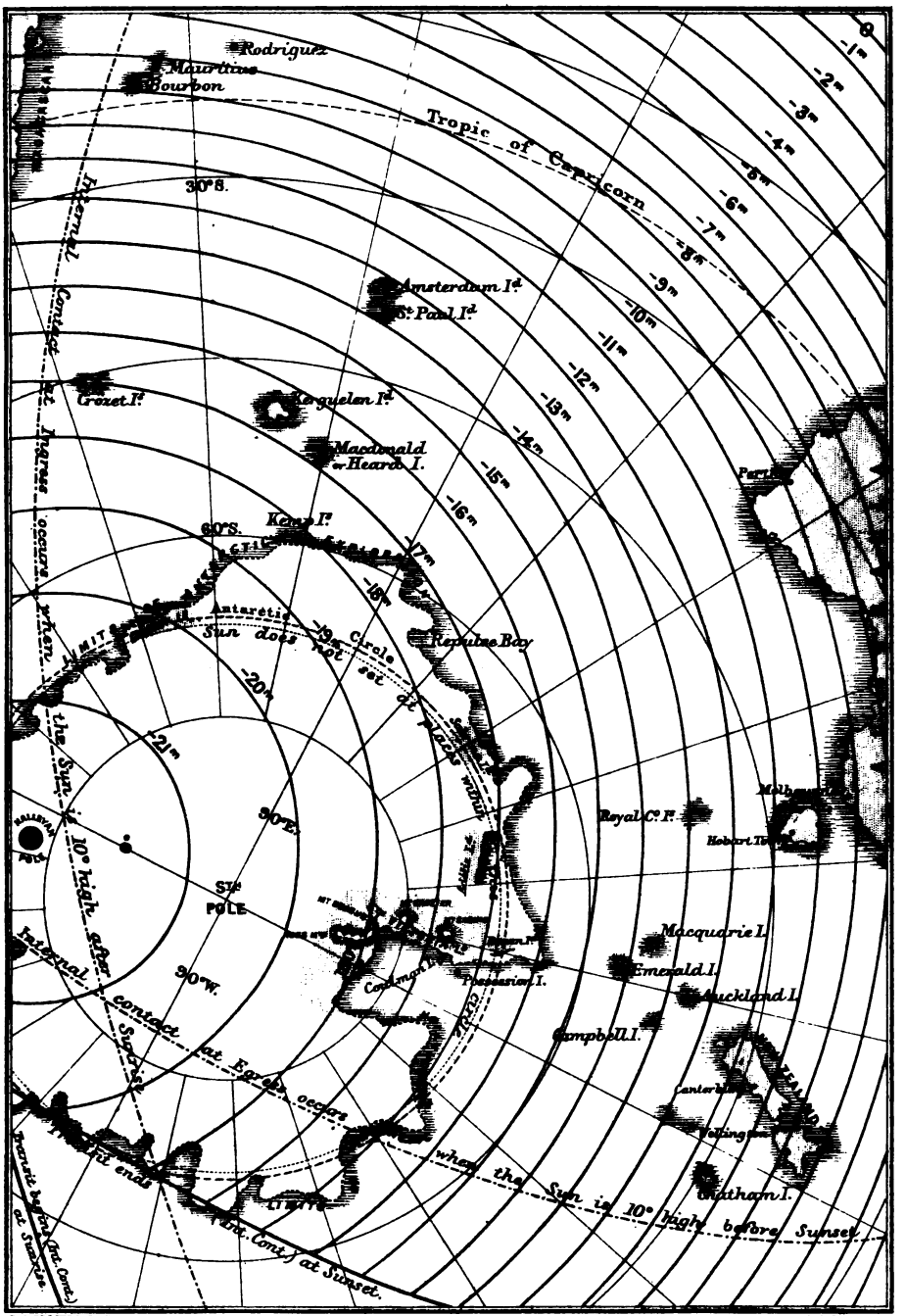
It will be perceived from the chart that Macdonald, or Heard Island (the only new observing station suggested in response to the advice of the Greenwich Board of Visitation), although well placed, is somewhat too near to Kerguelen Island to have favourable independent prospects of good weather. In other words, the occupation of this island will be useful, as increasing the number of Southern Halleyan stations and the value of the Southern observations as a whole, supposing weather to be favourable; but it is not a station which adds greatly to the probability of success, so far as success depends on conditions of weather.

I cannot conclude the statement of my views respecting the approaching transit without renewing my expression of regret that the transit of 1874 should not have been correctly viewed from the beginning by the persons responsible for England's action in astronomical matters. We do not know precisely what would have happened in that case, though we can infer from what was (mistakenly) suggested as proper for the transit of 1882, that a certain

TRANSIT OF VENUS IN 1874.

CHART OF THE ANTARCTIC AND SUBANTARCTIC REGIONS WHERE THE DURATION OF THE TRANSIT WILL BE SHORTENED.

Plate XIII.



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$\underline{-18^m}$ Signifies that the duration is 18^m less than the Mean at all points along the curve so marked.

course would have been pursued which could not but have reflected credit on this country, both as respects scientific zeal and the spirit of enterprise. That an unfortunate mistake, admitted too late, should have led to such an anomalous state of things that British official astronomy, as well as British nautical authority, find it necessary to make little of opportunities whose importance they once fully and publicly recognised (in a precisely corresponding case), is assuredly most grievously to be regretted. To the earnest student of science it must also be a cause of serious regret that such opportunities should be wasted. But while as an Englishman and as a student of science I must needs share in these regrets, yet in a personal sense I have every reason to be satisfied. *Whatever justification my researches and appeals may have seemed to require has been afforded by the unanimous vote of the leading British astronomers assembled at the Greenwich Board of Visitation.* Those astronomers, doubtless, feel, as I have long felt, that it is hereafter, and not perhaps till many years hence, that final judgment will be formed on the matters which have been under discussion.

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